PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCXV.

PART I.

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MDCCCXV.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1814, to JAMES IVORY, Esq. for his various Mathematical Communications, printed in the Philosophical Transactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *Additional observations on the optical properties and structure of heated glass and unannealed glass drops. By* David Brewster, *LL. D. F. R. S. Edin. and F. S. A. Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read November 10, 1814.

Dear Sir,

Edinburgh, April 8, 1814.

In a former paper on the optical properties of heated glass and unannealed glass drops, I have briefly described the leading phenomena which they exhibit in their action upon polarised light. These experiments I have frequently repeated with the same results, and I have the satisfaction also of stating, that as soon as they were known in France, they were repeated and verified by M. Biot of the National Institute, to whose active genius this branch of optics owes great obligations.

Having ascertained that glass melted and suddenly cooled, possessed all the optical properties of crystallized bodies, I was anxious to determine if it exhibited any other marks of

a crystalline structure. Upon examining the bulb of an unannealed drop AB, Pl. I., fig. 1, by holding it between the eye and a sheet of white paper, I observed a number of lines converging to the vertex *a*, as represented in fig. 2. This structure was more or less apparent in every bulb which I examined, but never appeared in annealed drops. It exhibited itself even on the surface, and seemed to be owing to an imperfect crystalline form, yet it was not marked with sufficient distinctness to entitle me to consider it as the effect of crystallization. In one specimen, however, where the bulb AB remained unshattered, while all the rest of the drop was burst in pieces, the lines diverging from *a* were most distinctly marked, and the bulb was actually cleft in the direction of these lines, so as to produce a real dislocation at the *surface* of the drop. We may therefore consider the drop as possessing that crystalline structure which gives cleavages in the direction of lines diverging from its apex. By examining the fragments of the drop after it is burst, another cleavage is distinctly perceptible: it is parallel to the outer surface, and produces a concentric structure like that of an onion. This cleavage also shows itself in the splinters which are detached from the surface of the drop when it is ground upon freestone. A third cleavage is visible in the direction of lines inclined to the axis of the drop, as represented in fig. 3; but it is not so distinct as the two first.

As it appeared probable that the glass drops possessed a less degree of density than if they had been annealed, I attempted to ascertain this point by measuring their specific gravities in these two different states. The unannealed drops, however, had always one or more vacuities, such as E, F,

fig. 1, so that I was able to obtain only approximate results by estimating the magnitude of these cavities.

The following specific gravities were measured by my friend Mr. JARDINE, with his usual correctness.

Unannealed flint glass drop, fig. 1,	-	3.20405
Annealed flint glass from the same pot		3.2763

In order to correct the first of these measures, I moulded a piece of bees' wax into the size and form of the cavities E, F, fig. 1, by examining them under a fluid of the same refractive power as the glass. I then formed the two pieces of wax into a sphere, and thus ascertained, with tolerable accuracy, the weight of a quantity of water of the same magnitude as the cavities. By this means I obtained the following measure,

Corrected specific gravity of the unannealed drop 3.264, a result differing so little from that of the annealed glass, that we may consider them as having nearly the same density.

With the view of obtaining some farther insight into the structure of the crystallized drop, I brought the one, represented in fig. 1, nearly to a red heat. Its shape suffered no change at this temperature, and the vacuities E, F, still remained; but it had now lost the faculty of depolarisation, and the particles had therefore assumed a new arrangement. By increasing the temperature, the cavities E, F, disappeared: the lower side of the drop, upon which it rested, was indented by the bottom of the crucible; but it had in no other respect lost its external shape, the appearance of the cleavage in fig. 2 remaining unaltered. In this state Mr. JARDINE measured the specific gravity of the drop, and found it to be 3.278, which is almost exactly the same as that of the annealed drop.

In order to observe the manner in which the cavities disappeared, I suspended one of the drops by a wire, and viewed it with a telescopic microscope when exposed to a strong heat. Soon after the drop became red hot, the cavities gradually contracted, and at last vanished, the centre of the cavity being the part that was last filled up. The drop had begun to melt at its smaller extremity, but the lines represented in fig. 2 were still visible, the heat probably not having been sufficiently intense to affect its superficial structure.

As the specific gravity of the crystallized drop is nearly the same as that of the annealed drop, the cavities must be produced by the contraction which the internal part experiences in cooling, for the sudden induration of the outer layer prevents the contraction from taking place in any other way. The manner, too, in which the cavities disappear, is a complete proof that they contain no air, and hence we may consider their magnitude, which increases with the size of the drop, as a measure of the contraction which the glass undergoes in its transition from the temperature at which it melts, to the ordinary temperature of the atmosphere.*

I am informed by Dr. Hope, that he has frequently obtained unannealed drops of crown glass, in which there were no vacuities, and that they all burst spontaneously in the course of a few months. As there is at present no crown glass manufactory in this part of Scotland, I have not been able to make any experiments with drops of this kind; but there is every reason to believe that they would exhibit the same optical properties, as those which are formed of flint

* Upon this supposition, the contraction of glass in bulk, in passing from the first of these states to the second, will be $\frac{1}{45.3}$ or 0.02205.

and bottle glass; and that the contraction of the internal parts, in consequence of which the vacuities are produced, is not necessary to that arrangement of particles upon which these properties depend. In the flint glass drops, such as ABC, fig. 1, there is sometimes only one vacuity, in the thick part at E, and as the slender extremity C is perfectly cold before the vacuity E is formed, and when the glass round E is red hot, it is obvious that the part C has suffered no contraction, and is in the same state as the crown glass drops obtained by Dr. Hope. But the extremity C has a more perfect structure than the bulb AB, as it possesses distinct neutral axes: hence we may infer that the crown glass drops, without vacuities, will exhibit neutral axes in every part of their length; that their structure is more uniform than that of flint and bottle glass drops; and that the difference between the specific gravity of the drop, and that of the annealed crown glass from which it is made, will afford a correct measure of the contraction which the glass experiences, in passing, by a gentle gradation of temperature, from the fluid to the solid state.*

* When a piece of red hot steel is plunged in cold water, it experiences a diminution of density analogous to that which takes place in drops of melted crown glass. Mr. R. Pennington found that a piece of steel, which, when soft, measured 2.769 inches had expanded to 2.7785 inches, after being hardened by immersion, when red hot, into cold water. Mr. Cavallo gives the following measures, without mentioning by whom the experiment was made.

Specific gravity of soft steel hammered - -	7.840
——————— of soft steel hammered, and hardened in water	7.818.

In all these cases the particles are held in a state of unnatural constraint by the sudden induration of the external coat; and therefore it is probable, that neither the glass nor the steel will expand by any moderate accession of temperature. If this conjecture be well founded, it will enable us to supply one of the greatest desiderata in the arts, a *pendulum of invariable length*.

The contraction of the flint glass drops, as computed from the magnitude of the cavities, must always err in defect; but the maximum result obtained from a considerable number of drops may be regarded as a tolerably correct measure of the diminution of density.* For this purpose, those drops should be employed in which the cavities are numerous, and scattered over every part of their length. The largest drops have generally this character, and I have one of these in my possession, in which there are no fewer than *seven* cavities.

A considerable degree of difficulty is experienced in procuring unannealed drops of flint glass. Owing, I presume, to the softness of this kind of glass, the greater number of the drops burst as soon as they are cooled, and from some pots of glass I have been able to procure only four drops out of twenty-four that were plunged into the water. One of these burst in my hand some hours after it was made, without any part of the tail having been previously broken, and another burst on the following day when lying on the table exposed to no change of temperature. The best method of obtaining the drops entire, is to watch the moment when the red heat disappears in the centre of the bulb, and to remove it instantly from the water.

As the minutest fragments of all crystallized bodies have the same action upon light, as the crystals of which they formed a part, I expected a similar property in the fragments

* The specific gravity of the unannealed drop, as corrected in page 3, from an estimate of the size of the cavities, approaches very near to that of the annealed drop. The difference is only 0.012, and would have been considerably less had the cavities been more numerous.

of the shattered drops. One of these, which was about the sixtieth of an inch thick, did not possess the property of depolarisation, and with more than twelve fragments of different thicknesses, below the thirtieth of an inch, I obtained a similar result. A fragment, however, of a crown glass tear, which had burst after being dropped into the water, and which was about two-tenths of an inch thick, depolarised light in every position, but did not exhibit any coloured rings by polarised light.

I have not been able to make a complete series of experiments on the effects of heat upon crystallized bodies, but it will appear from the following experiment that they are not likely to conduct us to new results. I heated to a great degree a fine crystal of spinelle ruby, which has not the property of double refraction, but it did not produce the least change upon a polarised ray. The crystal was $\frac{1}{28}$ of an inch thick; and a piece of crown glass of the same thickness, and brought to the same temperature, depolarised a considerable portion of light.

The effects of heat, as indicated by the preceding experiments, are, perhaps, too imperfectly developed to authorise us to draw those important conclusions, to which they seem so well calculated to conduct us. One of these, however, is so palpable, and so clearly deducible from the phenomena, that it must already have suggested itself, namely, the production of a *new species of crystallization* by the agency of heat alone. When light is transmitted perpendicularly through a plate of glass, the glass exercises no more action upon it, than if it were a mass of water. When the glass, however, is heated, the particles not only expand, but assume a new arrange-

ment, till at a certain temperature the crystallization is complete. As the temperature diminishes, the particles approach one another, and gradually recover their former arrangement. The crystallization which is thus produced in drops of melted glass, is rendered permanent by the sudden immersion of the drop in water, which arrests the particles in that particular position that constitutes the crystalline state of the body. Hence it follows that the particles of glass, when separated to a certain distance by the expansive energy of heat, assume a crystalline arrangement; and, unless they are fixed in this state, by a sudden diminution of temperature, the crystallization is gradually destroyed by the approximation of the particles which takes place during the operation of slow cooling.

During my experiments on depolarisation, which I shall soon have the honour of submitting to your notice, I discovered another species of crystallization, which is the effect of time alone, and which is produced by the slow action of corpuscular forces. This kind of crystallization appears, in general, to accompany the consolidation of many vegetable and animal products, and will probably be found to have had an extensive influence in those vast arrangements which must have attended the formation of our globe.

I have the honour to be,

Dear Sir,

your most obedient humble servant,

DAVID BREWSTER.

To the Right Hon. Sir Joseph Banks, Bart. K. B. P. R.S.

Fig. 1.

Fig. 2.

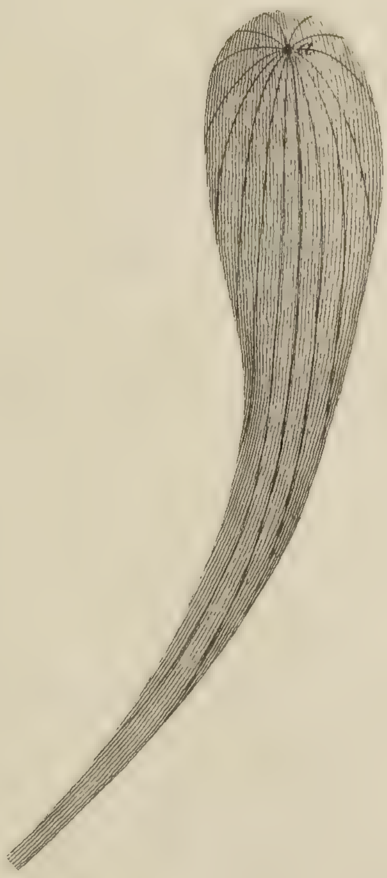
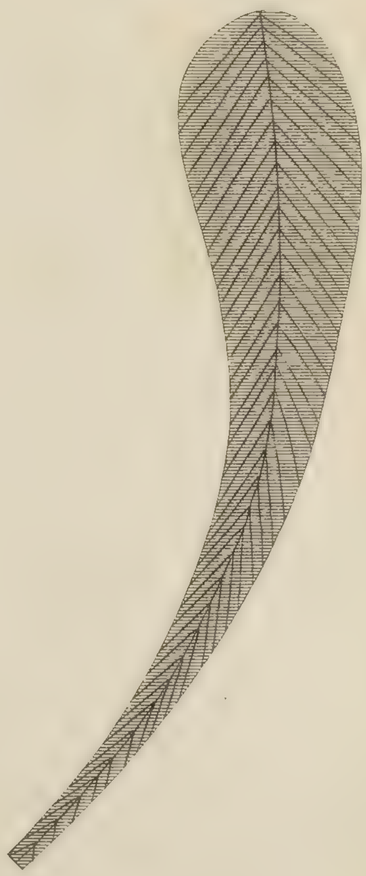


Fig. 3.



P. Basire sc.

II. *Description of a new instrument for performing mechanically the involution and evolution of numbers. By* Peter M. Roget, *M. D. Communicated by* William Hyde Wollaston, *M. D. Sec. R. S.*

Read November 17, 1814.

To abridge that species of mental labour which is required in conducting arithmetical computations, has been the professed object of a variety of mechanical contrivances. But the greater number of arithmetical machines, as they have been called, are more ingenious than really useful, and have been recorded more as objects of curiosity, than as admitting of convenient or ready application in the actual practice of arithmetic. The machine invented by PASCAL, and others constructed on the same principle, were, strictly speaking, limited to the simpler operations of addition and subtraction, and were incapable of being applied to the finding of products or quotients in any other way than by effecting a number of successive additions or subtractions. Still less did they aim at the immediate performance of the higher operations of involution, which, even by the most compendious methods of arithmetic, is a laborious process; or of the extraction of roots, to which the common rules furnish but a circuitous and slow approximation.

The only instruments which promise to afford real assistance to the practical calculator, are those founded on the theory of logarithms: a theory, which has been the fertile

source, as well as the commodious instrument of discovery in every department of mathematical inquiry. The scale of GUNTER, and the common sliding-rule, are derived from the properties of logarithms; and the purposes to which they are immediately applicable are the *multiplication* and *division* of numbers. The instrument, of which I purpose giving an account to the Society in this communication, is founded on a particular mode of employing logarithms, and is calculated to apply immediately to the *involution* and *evolution* of numbers. To those who are already conversant with mathematical pursuits, a few words would suffice to explain the principle on which it operates: but to such as are not familiar with the practical employment of logarithms, or of the common sliding-rule, the following statement of the chain of reasoning on which they depend, may conduce to render the subsequent details more intelligible.

The mode in which logarithms are instrumental in facilitating computation, is by converting the more difficult and laborious into the simpler operations of arithmetic. This is effected by substituting, instead of the numbers on which the requisite operations are to be performed, other numbers previously calculated and arranged in tables, so that every number in the natural series, has one of these artificial numbers, or logarithms, corresponding to it. These logarithms are so calculated, that, by adding together those which correspond to any given factors, the sum obtained shall be the logarithm, or artificial number, corresponding to the product of these factors. By consulting the tables, therefore, this product may be discovered; for it will be the number answering to the logarithm, or sum, thus obtained. The

subtraction of one logarithm from another will, in like manner, give the logarithm of the quotient resulting from the division of the number corresponding with the second, by the number corresponding with the first. The multiplication of a logarithm by any number will change it into another logarithm, which will answer to that power of the number corresponding with the former logarithm, which has this multiplier for its exponent.

But it will be seen that even in the simplest and most direct applications of this invention some exertion of arithmetical skill and some share of mathematical knowledge are requisite. Even this species of labour may, however, be avoided by the employment of lines as the representatives of logarithms; so that by the simple admeasurement of these lines, with their sums, differences, or multiples, on a given scale, the result of any of the above mentioned operations may be obtained within a certain degree of accuracy. A farther improvement consists in graduating a line of a convenient length *logometrically*, that is, dividing it so that the distance of each division from the beginning of the line, which is marked with unity, shall measure, on a given scale of equal parts, the logarithm of the number which is affixed to it. A line so divided is known by the name of GUNTER's scale.

The divisions which are situated at equal distances, being marked by numbers whose logarithms have equal differences, it follows that the spaces intervening between any two numbers are proportional to the differences between their respective logarithms; or are measures of the ratios between each of these numbers. The same use may therefore be made of such a scale, as of a table of logarithms with regard to operations to

be performed on their corresponding numbers. Thus it will be found, that the portion of the scale extending from 1 to 3, added to that extending from 1 to 4, is equal to that between 1 and 12: showing that the logarithm of 3, added to that of 4, is equal to the logarithm of 12: or that the ratio of 1 to 3, added to that of 1 to 4, composes the ratio of 1 to 12: or that 12 is the product of 3 and 4. The excess of the interval between 1 and 24, over that between 1 and 6, or, what is the same thing, the interval between 6 and 24, will be equal to that between 1 and 4; showing, by a similar process of reasoning, that 4 is the quotient of 24 divided by 6. This comparison of the intervals between the numbers on GUNTER'S scale is effected with great ease by the addition of another scale, which may be called the slider, exactly equal in length to the former, and bearing the same divisions, but capable of being moved by its side, so as to allow of any part of the one being applied to any part of the other. In this form it constitutes the common sliding-rule, the utility of which is so generally known in resolving all questions that require the simple operations of multiplication and division, or relate to the finding of any term of a proportion. Supposing the two scales originally to coincide, the sliding scale being the undermost, by advancing the slider any given distance, each of its divisions will be brought under those of the fixed scale, which before were respectively situated farther forwards by an interval equal to that given distance. Every number in the upper scale will therefore have to the number standing under it on the slider, the same constant ratio; a ratio indicated by the number under which the unity, or commencement of the scale, of the slider has been placed. The former

numbers will therefore be the multiples of the latter by this constant number. Thus, by adjusting the slider, so that its unity shall stand under any given multiplier or divisor, the upper line will exhibit the series of the products of all the subjacent numbers by the given multiplier: and conversely, the slider will exhibit the series of the quotients resulting from the division of the numbers immediately above them by the given divisor.*

* As the practical mode of using the sliding-rule is frequently not obvious even to those who are in possession of the principle of its construction, I shall beg leave to point out the following proposition, as one that leads directly to the solution of every case to which the instrument can be applied, and an attention to which, therefore, may conduce to its more ready and more general employment. *In every position of the slider, all the fractions formed by taking the numbers on the upper line as numerators, and those immediately under them as denominators, are equal.* Thus every corresponding numerator and denominator, having to each other the same ratio, may be considered as two terms of a proportion. Any two of these equivalent fractions will therefore furnish the four terms of a proportion; of which any unknown term may be supplied, when the others are given, by moving the slider till the numbers composing the terms of the given fraction, are brought to coincide on the two lines. The required term will then be found occupying its proper place opposite to the other given term. Thus, from the proportion A : B : : C : D, we may derive $\frac{A}{B} = \frac{C}{D}$; and adjusting the slider so that B shall stand under A, D will be found under C, when C is given: or C will be found over D, when D is given. A similar process would have furnished A or B, when one of them together with C and D, were given. Since the products of each numerator by the denominator of the other fraction are equal; (that is, AD = BC) ; when one of the terms is unity, the question becomes one of simple multiplication or division. The product of A and B, which we may call P, will be found, as before, by placing the slider so as to express the fractions $\frac{A}{1} = \frac{P}{B}$. The quotient of A divided by B, which we may call Q, will in like manner be found by forming the fractions $\frac{A}{B} = \frac{Q}{1}$: that is, in the former case, the product P will stand over B, when the 1 on the slider is brought under A; and in the latter case, the quotient Q will stand over the 1 of the slider when B is brought under A.

This instrument has been variously modified with a view of enlarging its scale, or of adapting it to particular objects, such as the calculation of exchanges, the measuring of plane and solid bodies, and the computations of trigonometry. The Society has recently witnessed its successful application, by Dr. Wollaston, to another science, in his synoptic scale of chemical equivalents, for the invention of which every practical as well as philosophical chemist must acknowledge to him their deep obligation.

But to whatever purposes the sliding-rule may have been applied, its use is necessarily limited to those operations which are performed by the simple addition or subtraction of logarithms, and to the corresponding arithmetical operations above mentioned. It is not directly adapted to the multiplication or division of logarithms by any number, and therefore is not directly calculated to perform the involution or evolution of numbers, to which, as was before noticed, the multiplication and division of logarithms correspond. Yet many practical, as well as philosophical, inquiries occur, in which it is necessary to ascertain the powers and roots of numbers. In all researches, for example, which involve geometrical progressions, or exponential quantities, and whenever the terms of a series are to be computed in obtaining approximate solutions, these questions present themselves. The common sliding-rule furnishes no direct mode of determining even the simple power or root of a given number: and when the exponent of the required power or root is not an integral, but a fractional number, its inadequacy to resolve the question is still more apparent. The squares and square roots, it is true, are often pointed out on the common rules, by means of a supplemen-

tary line graduated so that each of its divisions are double in length to those of the two other lines. A line of cubes, or cube roots, or of any other given power, might, in like manner be subjoined. But it is obvious that the uses of any such additional lines are confined to cases where a particular power is concerned: they give us no assistance in the case of any other power or root, which has no immediate relation with the former.

A new mode of graduation has occurred to me which possesses these requisites, and exhibits, on simple inspection, all the powers and roots of any given number, to any given exponent, with the same facility, and in the same way, that products, quotients, and proportionals, are exhibited by the common sliding-rule. It is accordingly a measure of powers, in the same way as the scale of GUNTER is a measure of ratios. An example will best illustrate the principle of its construction. If it were required to raise the number 2.123 to the fifth power: availing ourselves of logarithms, we should multiply the logarithm of 2.123 (or 0.32695) by 5. The product (1.63475) would be found by the tables to correspond to 43.127, which, with decimals to three places only, is the number required, or the fifth power of 2.123. If the exponent, instead of an entire number, as 5, were fractional, as 4.3719, the operation of multiplying by such a number would be more tedious, and might evidently again be abridged by having recourse to logarithms. Taking, then, the logarithm

of - - -	0.32695 or	9.5144813
and the logarithm of -	4.3719 or	0.6406702
and adding them, we obtain -		0.1551525

a logarithm answering to the number 1.4294, the product we have been seeking. But this product is itself a logarithm, namely, the logarithm of the power required. The number having for its logarithm 1.4294, namely, 26.878, is therefore the power sought for, or $2.123^{4.3719}$.

It may be observed, in this last example, that of the numbers added together, the first was the logometric logarithm, (that is, the logarithm of the logarithm) of the given root: the second was the simple logarithm of the exponent; and the sum of these was the logometric logarithm of the power. If, therefore, we were at the pains to construct a table having three sets of columns; the first containing the natural series of numbers; the second, their corresponding logarithms; and the third, containing the logarithms of those logarithms; we should possess the means of raising any given number to any given power, by the simple addition of the numbers in the second and third columns; just as common multiplications are effected by the addition of common logarithms. It is evident that a line might be graduated so that its divisions should correspond to the numbers in the third column, or should represent the logometric logarithms of the numbers marked upon them: and if this line were applied so as to slide against another line logometrically divided, it would enable us to effect the very operation I have been describing, and thus give us, by inspection, the powers corresponding to any given root and exponent.

The instrument, then, in its simplest form, would consist of two graduated scales applied to each other. A portion of these scales is represented, Pl. II, fig. 1. The lower rule, AA, which I shall call the slider, is the common GUNTER's

double line of numbers, or is a line logometrically divided; the divisions of the first half being from 1 to 10, and being repeated on the second half in the same order. The upper or fixed rule, BB, is graduated in such a manner, that each of its other divisions is set against its respective logarithm on the slider; and, consequently, all the numbers on the slider will be situated immediately under those numbers in the upper rule, of which they are the logarithms. Thus 2 on the rule will be over 0.30103 of the slider; 3 over 0.47712: 2 on the slider will stand under 100 on the rule; 3 under 1000; and so on.

As the series of ordinary logarithms express the exponents of 10, of which the corresponding numbers are so many successive powers, it is evident that, in this position of the instrument, the upper line will exhibit the series of the powers of 10, corresponding to all the exponents marked on the slider. It will be seen, for instance, that the second power of 10 is 100, the third, 1000, &c.: that the 0.5th (or the square root) is 3.163; the 0.25th (or the fourth root) is 1.778; the 0.2th (or the fifth root) is 1.585: and so on.

In every other position of the slider, the upper rule will exhibit, in like manner, the series of powers of that number under which the unit of the slider has been placed, while the opposite numbers on the slider are the exponents of those powers. Thus, if (as in Pl. II, fig. 2) the unit of the slider be placed under the division 3 of the upper rule (at R); the square of 3, or 9, will be found over the 2 of the slider: its cube, 27, over the 3; its fourth power, 81, over the 4: and so on for any other power. It is evident, then, that in order to find a given power of any number, the unit of the slider

must be set underneath that number in the upper rule; and that the number sought will then be found above that number in the slider which expresses the magnitude of the required power.

Such being the mode of its application to the finding of powers, its use will be obvious in performing the contrary operation of finding roots. The root might, for this purpose, be considered as a fractional power: but as this would require a reduction to decimals, the easiest mode will be to place the number expressing the degree of the required root under the given number, and the root itself will then be found over the unit, or beginning of the scale, in the slider. For fractional powers, the denominator of the exponent must be placed under the root, and its numerator will then point out the power.

It is hardly necessary to add, that by the same mode we may discover the exponent of any given power to any given root: since, whatever be the root over the unit of the slider, the whole series of the powers of that root, with their corresponding exponents, are rendered apparent. This circumstance may indeed be considered as an additional recommendation to the employment of this instrument: for it affords to those less versed in the contemplation of numerical relations an ocular illustration of the theory of involution. It presents, at one view, the whole series of powers arising from the successive multiplication of all possible numbers, whether entire or fractional; and exhibits this series in all its continuity when the exponents are fractional, and even incommensurate with the root itself. The production of the upper line in one direction conveys a more accurate notion of the pro-

gressive and rapid increase of those powers, than can be acquired by mere abstract reflection: and its continuation on the other side, shows the slow approximation to unity which takes place in the successive extractions of higher and higher roots.

A variety of forms of construction might be given to instruments operating on the principle now explained. The following has appeared to me, on the whole, to be the most convenient for practical purposes; it is represented on a reduced scale in Pl. II, fig. 3. In order to preserve a sufficient magnitude of scale, I have divided the line of roots and powers into two parts; placing the one above and the other below, and interposing a slider with a double scale of exponents. The slider of the common sliding-rule is graduated in a way that is exceedingly well suited to this purpose, having divisions on each edge, and carrying two sets of numbers from 1 to 10. Adapting a blank ruler to one of these sliders, which must be fixed in a proper position, I mark off, on the upper line, the series of numbers against their respective logarithms on the slider; placing 10 over the middle unit of the slider, 100 over the 2, 1000 over the 3, and so on, proceeding towards the right from 10 to 10000000000, the tenth power of 10, an extent which is more than sufficient for all useful purposes. The space to the left is also graduated on the same principle, from 10 to 1.259 which is the tenth root of 10, or $10^{0.1}$. The upper portion of the rule being thus filled, I place the continuation of the same line on the lower portion, beginning on the right hand, and proceeding in a descending series of fractional powers of 10, corresponding with the exponents on the intermediate slider, which, when applied to this portion, are to be taken as only

one hundredth of their value when applied to the upper portion. While 1.259 therefore is marked on the right, 1.0233 $= 10^{0.01}$) will occupy the middle, and 1.002305 ($= 10^{0.001}$) the left end of the lower line. It is evident that the graduation might thus be continued indefinitely in both directions. But for all practical purposes the limits thus obtained will be found amply sufficient: for the well known property of the logarithms of roots in a descending series, enables us to dispense with all farther continuation of the scale in that direction. In proportion as numbers in a descending series approach very near to unity, their logarithms bear more and more exactly a constant ratio to the excess of those numbers above unity, namely, the ratio expressed by the modulus of the system, or 1 to .4342944819, &c. As we descend in the scale, therefore, the decimal part of the exponents becoming smaller and smaller, the corresponding logarithms will approximate so nearly to the multiple of that decimal part by this modulus, that no *sensible* error will result from assuming them to be the same.* The divisions to the left of the lower portion of the rule may therefore be taken as sufficiently accurate representations of the divisions which would occur in the succeeding portions of the line, if it were prolonged indefinitely in that direction.

The applications of which this instrument is susceptible are

* Thus the logarithm of 1.05 is .021189
that of 1.005 is .0021661
and of 1.0005 is .00021709
which differs from the product of the modulus by .0005 (or .00021715) by a quantity affecting only the fourth significant figure. The roots 1.0005, 1.00005, 1.000005, &c. may, therefore, without sensible error, be considered as coinciding with the division 217 on the slider.

various, and will easily present themselves. In many speculative and practical inquiries, cases occur in which geometrical progressions are concerned, and in which it becomes a question, the first term and the common ratio being given, to find the other terms; or, knowing the first and also any other term, to ascertain the rate of increase. In all these cases, it is obvious that the first term is to be regarded as the root, or first power, and the unit in the slider adjusted, so as to coincide with that number in the line of powers, that is, in the upper and lower portions of the fixed rule. The number of terms will constitute the exponent of the series, and the power corresponding to each successive exponent 2, 3, 4, &c. will be the second, third, fourth, &c. term of the progression.

The successive amounts of a sum placed at compound interest compose a geometrical progression; and accordingly all questions of compound interest are resolvable by this instrument. The rate of interest, or the per centage per annum, being added to 1, gives the amount of £1. at the end of one year. Thus, at 5 per cent. the amount is 1.05, at 3 per cent. 1.03, and so on. In either case this number is to be regarded as the first term, or root of the series. Setting the unit of the slider against this number on the rule, we shall find the amount of £1. at the end of $5\frac{1}{2}$ years, opposite to the number 5.5 on the slider, and the same of any other interval of time. If it be required to ascertain in what time a sum placed at compound interest at 3 per cent. would be doubled: placing the unit over 1.03, the number 2 on the rule will indicate 23.45 on the slider, as the number of years required for doubling the sum at that rate of compound interest.

Questions relating to the increase of population and to the

calculation of chances, involve the investigation of powers, which may be facilitated by this instrument. Examples of its application also occur in considering the reduction of temperature which bodies undergo by the communication of heat to surrounding bodies, the quantities of light transmitted through different thicknesses of a transparent medium: the diminution of density which the air in a receiver undergoes during its exhaustion by the air pump, and the relation of the density of the atmosphere with its elevation. The interpolation of a given number of mean proportionals between two given numbers, is sometimes required for the solution of a problem, and is easily effected by the rule above described. Thus, in dividing the musical octave into twelve equal semi-tones, the following series of numbers must be calculated, viz. $2^{\frac{1}{12}}$, $2^{\frac{2}{12}}$, $2^{\frac{3}{12}}$, $2^{\frac{4}{12}}$, $2^{\frac{5}{12}}$, $2^{\frac{6}{12}}$, $2^{\frac{7}{12}}$, $2^{\frac{8}{12}}$, $2^{\frac{9}{12}}$, $2^{\frac{10}{12}}$, $2^{\frac{11}{12}}$: this can readily be done in one position of the slider, for when the 12 marked on it is placed under 2 on the rule, the 1 of the slider will point to $1.0595 = 2^{\frac{1}{12}}$, the 2 of the slider will indicate $1.1225 = 2^{\frac{2}{12}}$, the 3, $1.1892 = 2^{\frac{3}{12}}$, &c.

When the first term of a progression is less than unity, all the succeeding terms, that is, all the powers of that fraction, continually decrease. Now all the numbers contained on the rule are above unity: but the terms of such a decreasing progression may yet readily be found by assuming, instead of the first term, its reciprocal, which, being above unity, will of course be contained on the scale. The powers of this reciprocal, will, in like manner, be the reciprocals of the required series, which will accordingly be determined without difficulty. Let the following question, for example, be proposed.

Assuming, that when light is transmitted through water, one half of the quantity that entered is lost by passing through seven feet of water:* how much will be intercepted by passing through three feet? In questions of this sort it must be recollected that it is the quantities of transmitted, and not of intercepted light, that are in geometrical progression. If 0.5 is transmitted by seven feet, $0.5^{\frac{3}{7}}$ will be transmitted by three feet. As 0.5 is not contained on the rule, we must take its reciprocal 2, of which the $\frac{3}{7}$th power, or 1.3023, is given by the instrument: this number being opposite to the 3 on the slider, when its division 7 is placed under 2 on the rule. The reciprocal of 1.3023 or .9892 is the quantity transmitted; and therefore .0108 the quantity absorbed by three feet of water.

A variety of propositions relating to the general theory of logarithms are illustrated by this instrument. The assumption of the number 10, as the basis of our system of logarithms is arbitrary, and is chosen only for the sake of greater convenience in computation. The hyperbolic system, which has the number 2.302585093, &c. for its basis, possesses other advantages, especially in the higher branches of analysis. The instrument may be made to exhibit at one view the series of any particular system of logarithms, that is, of a system with any given basis, or any given modulus, by merely setting the unity of the slider against the given basis on the rule: or the given modulus on the slider against the number 2.7182818, &c. on the rule. The divisions on the slider will then denote the logarithms of the numbers opposed to them on the rule.

* YOUNG's Lectures on Natural Philosophy, I. 409.

Let it be required to determine the particular system of logarithms, in which the modulus shall be equal to the basis. Take out the slider, and introduce it in an inverted position, so that the numbers on it shall increase from right to left: and place the number .4343, &c. (the modulus of the common system) under 10 (its corresponding basis) on the rule, as represented in Pl. II, fig. 4. We shall find that in this position, all the other numbers on the slider will be the moduli corresponding to the respective bases of each different system, on the rule. Thus, the 1 on the slider, or the modulus of the hyperbolic system, is opposite to 2.718, the basis of that system. On the other hand, the division 2 on the rule is opposite to 1.4427, which is the modulus of the system having for its basis the number 2. Carrying the eye still more to the left, and observing the point where similar divisions appear both on the rule and the slider, we shall find it to be at the number 1.76315, which therefore expresses the modulus and the basis in that particular system in which they are both equal. The reason of the above process will readily appear when it is considered, that the modulus of every system is the reciprocal of the hyperbolic logarithm of its basis.

This inverted condition of the slider will also afford an easy method of solving exponential equations, for which there exists no direct analytical method. The following may serve as an example. Let the root of the equation $x^x = 100$ be required. Set the unit of the inverted slider under 100 on the rule, and observe, as before, the point where similar divisions coincide; this will be at 3.6, which is a near approximation to the required root: and accordingly $3.6^{3.6} = 100$.

The principle of the instrument above described might be

applied in a variety of different forms to these several purposes: and I shall beg leave to notice one or two that offer some peculiarities. If to the upper scale, which we may suppose to be fixed, and to be graduated logometrically, constituting, as we have already seen, the line of exponents, a slider be adjusted, graduated on both edges, according to the logometric logarithms; and the line below, which like the upper one is supposed to be fixed, be graduated in the same manner as the slider, the instrument will possess the following property. When the division 10 on the slider is set against any particular number, or exponent, in the upper line, all the numbers on the lower line will be the powers, to the same degree, of the numbers opposite to them on the slider: the degree of the power being marked by the exponent on the upper line which is above the 10 on the slider. The lower line, therefore, will exhibit the whole series of similar powers belonging to all possible roots; and conversely, the slider will exhibit all the roots of the same dimension, with regard to all possible numbers. Thus, if the 10 on the slider be under 3 in the line of exponents, it will itself be above 1000 (which is its cube) in the lower line; all the other numbers in that line will be the cubes of their opposites on the slider; and, conversely, the former will every where be the cube roots of the latter. This will be sufficiently apparent, when it is recollected that the addition or subtraction of logometric logarithms answer to the multiplication or division of simple logarithms, and therefore to the involution and evolution of numbers. The rule in this form, therefore, bears a closer analogy to the common sliding-rule; since in every position it exhibits the series of similar powers and roots, exactly in

the same way as the latter exhibits the series of similar products and quotients.

I have also contrived another form of the instrument which possesses some advantages in theory, though its execution may perhaps be more difficult. It is evident that the whole scale may, like Gunter's line, be thrown into a circular form; and this I have done in the way represented in Pl. III. The circle on the outside, being logometrically divided from 1 to 10 round the circumference, will constitute the line of exponents. The line of powers, being disposed in a spiral, will occupy the interior space, which may be made to revolve within the former, and should be provided with one or more threads extending from the centre to the circumference, and serving as radii to mark the position of all the parts of the spiral line with regard to the divisions of the outer circle. One of these threads may be fixed at the unit or beginning of the scale, and will serve to mark the position for the root of any required power. The spiral itself must be graduated exactly as the upper line in the first described rule: that is, the situation of the division 10 must be first determined upon, and then brought under the unit in the circle of exponents, that is, under the fixed thread. Every other division must then be marked with reference to the place of its logarithm on the circle, or must be made to occupy the same angular distance from the thread. This graduation will be most conveniently made by means of the moveable leg of a sector revolving on the centre of the circle. The comparison of the divisions of the spiral with those of the circle, may be made, either with this moveable sector, or with the threads already mentioned. The numbers on the spiral will increase

as they recede from the centre, and each turn will carry on the powers to an exponent 10 times higher than the preceding: and the converse will obtain with regard to the descending portion. Thus, immediately in a line with the 10, on the superior branch of the spiral, is found the number 10000000000, or 10^{10}: below it on the inferior branches, we find successively $1.258926 = 10^{0.1}$, $1.023293 = 10^{0.01}$, $1.00230524 = 10^{.001}$, $1.000230285 = 10^{.0001}$, $1.0000230261 = 10^{.00001}$, &c. of which, agreeably to the remarks that were formerly made, the decimal figures approach nearer and nearer to 2.302585093, &c. the reciprocal of the modulus of the logarithmic system.

A much greater extension might be given to the scale, by multiplying the number of turns of the spiral corresponding to the decuple increase of the exponents: but the superior accuracy thus obtained would probably be overbalanced by the diminished conveniency of application.

It is possible to exhibit in one view the whole series of roots, powers, and exponents, in all their possible relations, by the following disposition of lines. Let the lines AB, AC, (Pl. IV.), which I shall call respectively the line of exponents, and the line of roots, be drawn at right angles to each other, and a diagonal AD, or line of powers be drawn, bisecting this angle. Divide AB logometrically, so that the unit of the scale shall be at A: upon the same scale, divide AC into logometric logarithms, and AD into similar parts, by perpendiculars from the divisions of AC.

Through all these points of division, let there be drawn perpendiculars to the respective lines: and let each of these perpendiculars be considered as referring always to the numbers on the lines from which they are drawn. The following

relation will be preserved between the numbers belonging to any three of these perpendiculars that meet in one point; viz. that the number of the perpendicular to the line of roots, raised to the power expressed by the perpendicular to the line of exponents, shall be equal to the number denoted by the perpendicular to the line of powers. To find, for example, the second power of 3; following the perpendicular from the division 3 on the line of roots, and that from the division 2 on the line of exponents, till they meet in the point *e*, we find among the other set of perpendiculars, the line *fg* passing through the same point, which, followed till it meets the line of powers, indicates on it the number 9, which is accordingly the second power of 3. A similar process in other directions will furnish the root when the power and exponent are given, or the exponent when the root and power are given. We may thus perceive, at a single glance, not only all the powers of any particular root, and all the roots of any particular power; but also all the exponents of the series of powers belonging to the same root, as well as the similar powers of every possible root.

It is, perhaps, superfluous to observe, that the same method is applicable to the common scale of GUNTER; and that a table constructed accordingly, by dividing the sides as well as the diagonal logometrically, and applying three sets of perpendiculars, would, by their intersections, exhibit in one view all possible products and quotients resulting from all possible factors or divisors.

Fig. 1.

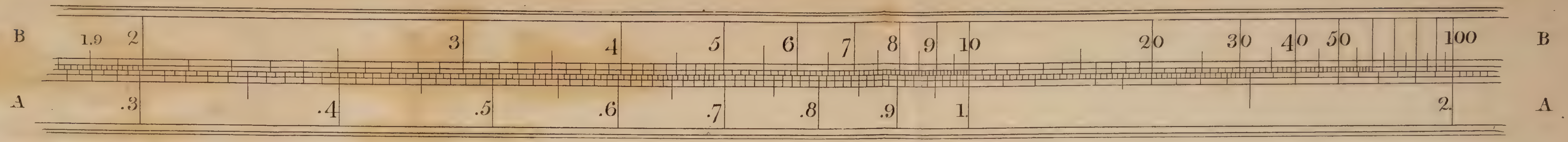


Fig. 2.

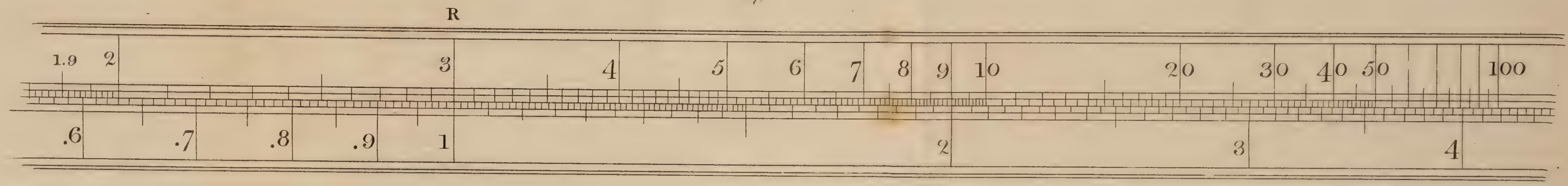


Fig. 3.

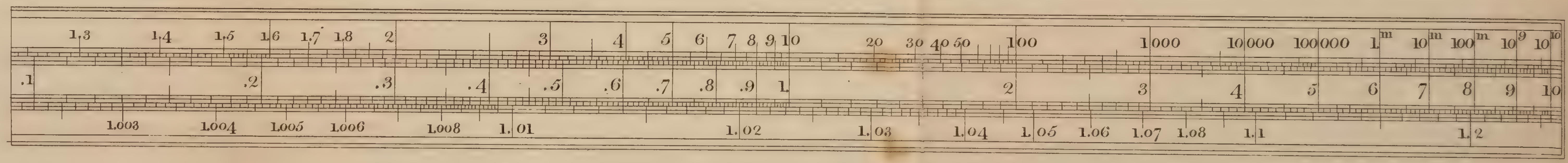


Fig. 4.

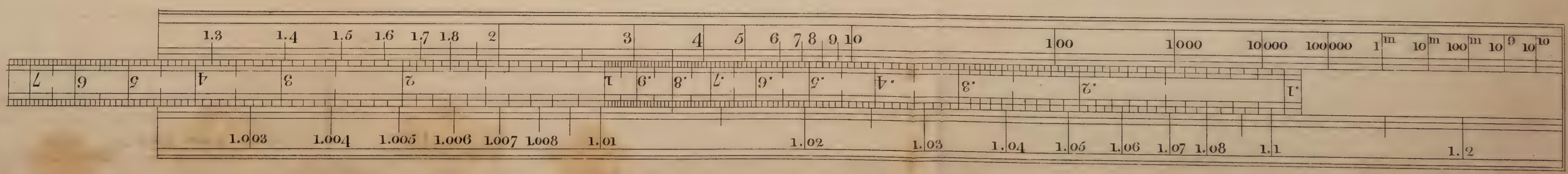


Fig. 5.

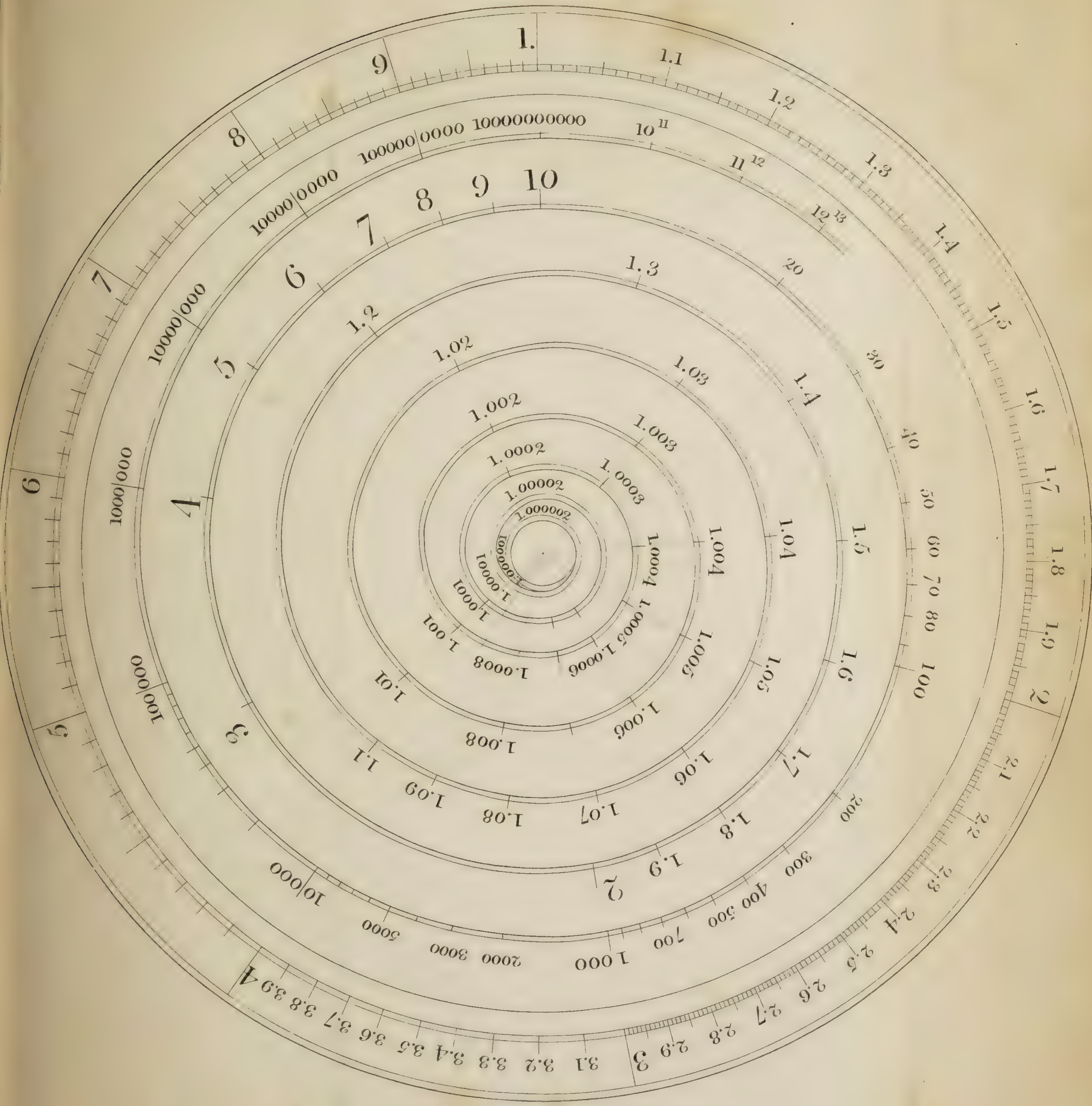

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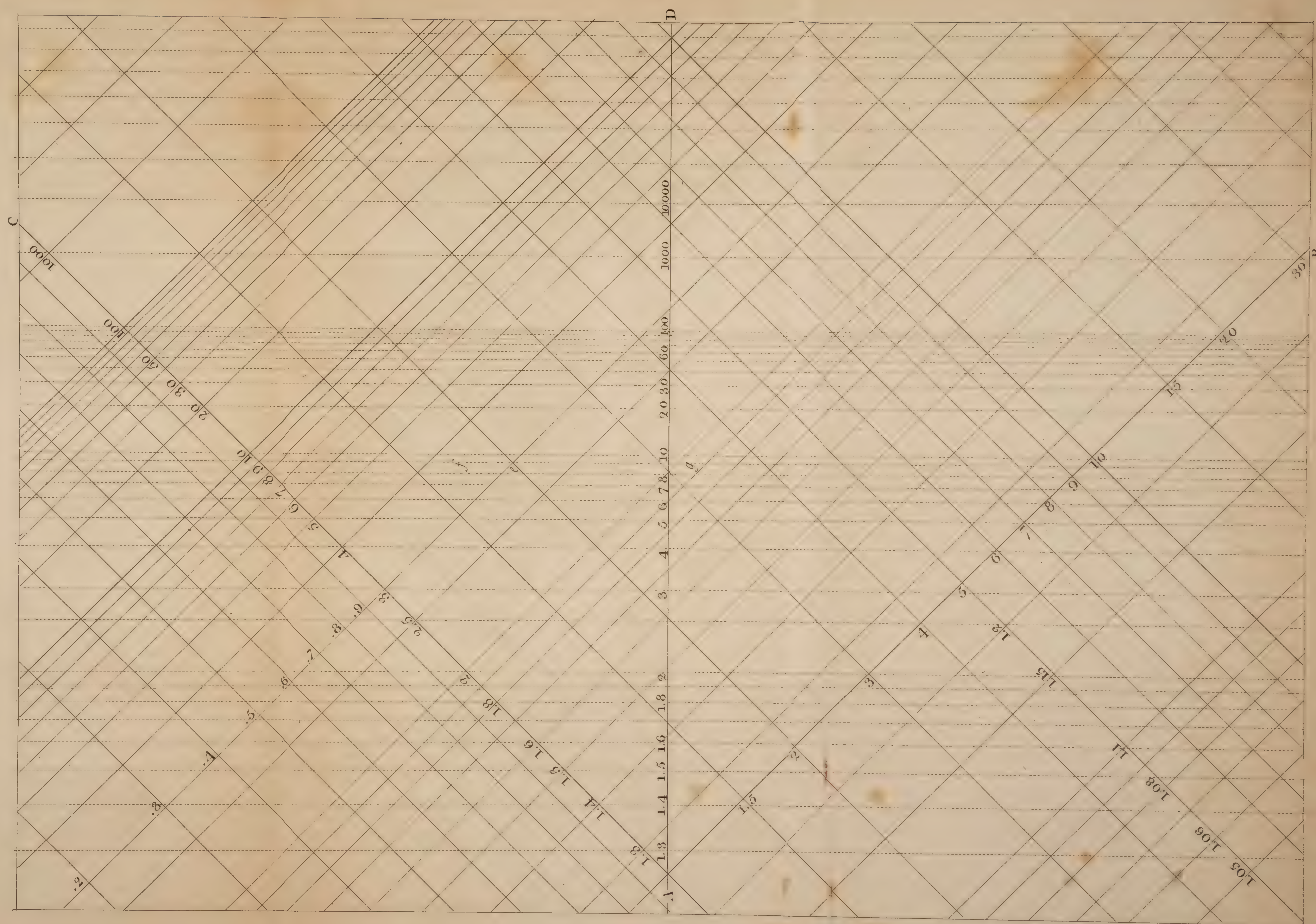
Fig. 6.



Jas. Basire sc.

III. *Experiments on the depolarisation of light as exhibited by various mineral, animal, and vegetable bodies, with a reference of the phenomena to the general principles of polarisation. By* David Brewster, *LL. D. F. R. S. Edin. and F. S. A. Edin. In a letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read December 15, 1814.

Dear Sir,

Towards the end of the year 1812, when I was engaged in examining the light transmitted through diaphanous bodies, I discovered the property which many of them possessed of *depolarising* the rays of light, or of depriving them of the polarity which they had received, either by reflection from the surface of a transparent body, or by transmission through a plate of agate. A short account of these experiments, which were exhibited to many of my friends in Edinburgh, was soon afterwards published in my treatise on new philosophical instruments.

As this singular property was possessed by numerous substances that exhibited no marks of double refraction, and even by animal and vegetable products, such as horn, tortoise-shell, and gum Arabic, it appeared necessary to distinguish it by a new name, and to refer it to a species of crystallization different from that of doubly refracting crystals. The circumstance, however, of agate and Iceland spar possessing

the faculty both of polarising and depolarising light, and the constant relation in the position of the axes which regulated these apparently opposite actions, induced me to think that the two classes of phenomena had the same origin. This opinion was afterwards strengthened by an experiment with a bundle of glass plates, in which light was depolarised by polarising it in a new plane; but in applying the principle to other phenomena, I was baffled in every attempt to generalise them. By extending, however, and varying the experiments; by examining the optical properties of every substance which I could command, and by comparing their structure with the phenomena which they exhibited, I have been led to the general principle to which they all belong, and to a series of results which, from their very nature, could not easily have been established by direct experiment. These conclusions, independently of their optical consequences, are peculiarly interesting to the chemist and the natural philosopher, by disclosing the structure of organised substances, and exhibiting new relations among the bodies of the animal, the vegetable, and the mineral kingdoms.

In proceeding to illustrate this subject, I shall first give some account of the experiments on which the theory is founded, and then explain the theory itself, and the conclusions which it seems to involve.

I. *Experiments on the depolarisation of light.*

I have already explained, in a former paper,* the general phenomena of depolarisation, and have shewn that almost all regularly crystallized bodies, such as plates of mica, of calca-

* Philosophical Transactions, 1814, Part I, p. 199.

reous spar, and of topaz, have two *neutral* axes at right angles to each other, and two *depolarising* axes at right angles to each other, and forming angles of 45° with the neutral axes.

In *mica*, the neutral axes coincide with the diagonals of its primitive rhomboidal base.

In *calcareous spar* the neutral axes are coincident with the diagonals of any of its rhomboidal faces, while the depolarising axes are parallel to the sides of these faces; but when polarised light is transmitted along the short diagonal of the rhomb itself, there is no position in which it is depolarised, or in which the two images continue visible during the whole revolution of the prism of calcareous spar next the eye. The evanescent image is no doubt restored by the interposition of the rhomb, but this only shifts the vanishing place of the images which will continue to disappear alternately in every quadrant.

In *topaz*, the neutral axes coincide with the diagonals of the base of its primitive right prism, and the depolarising axes are parallel to the sides of any of its square faces.

The minerals, in which I have not found the property of depolarising light, are *muriate of soda*, or rock salt, *fluor spar*, and a crystal of *spinelle ruby*. I have cut numerous plates in different directions from a cube of muriate of soda, and have tried many specimens of fluor spar, but in none of them could I discover the least trace of depolarising axes.

The only mineral which depolarises light in every position, and therefore possesses no neutral axes, is the *diamond*. Out of *fourteen* specimens of this gem which I examined, *seven* depolarised light in every position; *four* did not depolarise light at all; *one* depolarised about a fifth part of the light,

one about half of the light, and *one* nearly the whole of the light.

Semi-opal, like the diamond, depolarises light in every position; but there is an obvious approximation to neutral axes.

In employing another class of bodies, we are presented with a series of very singular results, which not only develope new affections of light, but lead to important conclusions respecting the crystallization of organised and unorganised bodies.

1. *Gum Arabic.* This vegetable substance, which is formed by concentric coats, has no neutral axes; but depolarises light in every position. In a very thin chip, however, the neutral axes are distinctly visible. With a strong solution of gum Arabic in water, I formed a thin film of it upon a plate of glass. In two or three days it became very hard, but though I have kept it six months, it has not acquired the property of depolarising light.

2. *Gum from the cherry tree.* A plate of this gum about one-twentieth of an inch thick, and so soft as to yield to the gentlest pressure of the nail, depolarises light, but the image does not wholly vanish in the neutral axes. Another plate of the gum, but exuded from a different part of the same tree, and much softer than the former, depolarises only a small portion of light.

3. *Caoutchouc.* This gum is composed of concentric or parallel layers of a vegetable juice, which are successively indurated by exposure to the sun. When a thin plate of it is made transparent by pressure between two plates of glass, it exhibits no neutral axes, but depolarises light in every position, in whatever direction the film is cut from the mass.

When the film was so extremely thin, that it could not be made transparent by pressure between two plates of glass, I stretched it over the mouth of a tube upon a piece of plane glass, and prevented it by means of a cord from recovering its shape. A layer of Canada balsam being then placed both below and above the caoutchouc, and another plate of glass laid upon the upper layer of balsam, the film became perfectly transparent. When exposed to polarised light, it exhibited *neutral axes*, like the most perfect crystals.

When caoutchouc is dissolved by heat, it loses completely its property of depolarisation, but it gradually recovers its former structure, and after a certain number of days, it is again capable of depolarising light. A piece of caoutchouc, which had been melted by heat, resumed its faculty of depolarisation at the end of twenty-five days; but upon pressing it gently with my finger, its structure was again destroyed, and at the end of nineteen days, it depolarised a small portion of nebulous light. Another piece of caoutchouc, dissolved by heat, had not recovered its crystalline state at the end of six days. After standing eighteen days, it depolarised a considerable quantity of light, and at the end of five or six weeks, it was capable of restoring the whole of the vanished image.

4. *White wax.* When a piece of wax is melted and cooled between two plates of glass, or when it is merely pressed between them by the heat of the hand, it depolarises light in every position. The restored image, however, has a nebulous appearance.

5. *Rosin and white wax mixed.* Rosin alone has not the property of depolarising light. When it is mixed with an equal part of white wax, and is pressed between two plates of glass

by the heat of the hand, the film is almost perfectly transparent by transmitted light, though of a milky white appearance by reflected light. It has not the property of depolarisation when the polarised pencil is incident *vertically*, but it possesses it in a very perfect manner at *an oblique incidence*, and exhibits the segments of coloured rings.

6. *Cells of the bee.* The waxen partitions of the honey-comb, when rendered transparent by layers of Canada balsam, depolarise light in every position.

7. *Manna.* This substance, when melted by heat between plates of glass, depolarises light in every position.

8. *Camphor.* A small piece of this substance when pressed between two plates of glass, without the application of heat, depolarises light in every position.

9. *Balsam of Tolu.* When a thin plate of this substance is formed between plates of glass with the assistance of a gentle heat, it depolarises light in every position; and a considerable degree of heat is necessary to deprive it of its crystalline structure. When it is allowed to cool very slowly, it does not acquire the property of depolarisation.

10. *Withered film at the root of the Calla Ethiopica.* This vegetable film depolarises light, and possesses distinct neutral axes which are parallel and perpendicular to the stalk of the plant, or to the parallel veins in the film.

11. *The fibres of flax, hemp, and cotton.* These vegetable fibres depolarise light, and have perfect neutral axes parallel and perpendicular to the axes of the fibres.

12. *The thin white semi-transparent leaf of the sea-weed* depolarises a small portion of light, and its neutral axes are parallel and perpendicular to the axis of the leaf.

13. *Adipocire from muscular fibre.* This substance melts as easily as white wax. It depolarises light in every position, and crystallizes differently from wax, manna, and spermaceti.

14. *Adipocire from the burying ground of the church des Innocens at Paris.* This substance melts at as low a temperature as the preceding, and when cooled between two plates of glass, it crystallizes in concentric rings which appear through the microscope like clusters of islands in a map, surrounded with engraved circles. It has no neutral axes, but depolarises light in every position.

15. *Adipocire from biliary calculi.* This body crystallizes exactly like benzoic acid, which it resembles in appearance, shooting out spicula at angles of about 20°. It requires a very considerable heat to melt it; crystallizes rapidly, and depolarises light in every position.

16. *The benzoic and oxalic acids,* when melted by heat, and then cooled, depolarise light in every position.

17. *Spermaceti.* This substance, when melted and cooled, depolarises light in every direction; and when pressed between two plates of glass without the aid of heat, it exhibits traces of coloured rings by polarised light.

18. *Gold beaters' skin.* Having procured a remarkably thin film of this animal substance, I rendered it transparent by placing it between layers of Canada balsam. It possessed the property of depolarising light, but it did not restore the whole of the evanescent pencil. Its neutral axes were not so perfectly developed as in the thin film of caoutchouc.

19. *Transparent and common soap.* A plate of transparent soap of any thickness depolarises light in every position; and

a thin film of the common yellow and white soap has the same property.

20. *Human hair.* The fine transparent hair of a child depolarises light, and possesses the most perfect neutral axes. These axes are parallel and perpendicular to the axis of the hair.

21. *Bristles of a sow* possess the same properties as hair. The neutral axes are seen more distinctly in this than in the preceding experiment, on account of the greater magnitude of the bristles.

22. *The fibres of silk and wool* depolarise light, and have neutral axes parallel and perpendicular to the axes of the fibres.

23. *The silkworm gut* and *sheep gut* depolarise light. In the former the neutral axes are perfectly developed, but in the latter there is merely an approximation to them.

24. *The human cuticle* depolarises light in every position.

25. *Parchment* depolarises light in every direction. In two positions, at right angles to each other, the restored image is indistinct and principally nebulous; while in other two positions at right angles to each other, at angles of 45° with the former, the restored image is distinct.

26. *The horny excrescence* on the human foot depolarises light in every position.

27. *The transparent film* at the joints of the claws of the common Partan depolarises light; and has neutral axes parallel and perpendicular to the length of the claw.

28. *The human nail* depolarises light in every position.

29. *A quill,* and the *thin film* which lines the inside of it, depolarise light. The former exhibits coloured rings, and the latter has distinct neutral axes.

30. *The cartilaginous breast bone of a chicken* depolarises light, and has neutral axes parallel and perpendicular to the longitudinal direction of the bone.

31. *The transparent cartilage* from the shoulder of a sheep has neutral axes, and produces the coloured rings by polarised light.

32. *The transparent edge* of the small fibres which compose the feathery part of a *quill* depolarise light, and have their neutral axes parallel and perpendicular to the axis of the fibre. It forms also an extraordinary prismatic image of the candle by transmitted light.

33. *The down of goose and ostrich feathers* depolarise light. Feathers from the neck and tail of a cock have neutral axes parallel and perpendicular to the direction of the fibres.

34. *Flat bones of a cod.* These bones depolarise light in every position, and exhibit coloured rings by polarised light. The soft cartilaginous substance, which is sometimes connected with them, possesses the same properties.

35. *Cylindrical bones of fish.* These bones depolarise light, and have their neutral axes parallel and perpendicular to the axis of the cylinder. They exhibit also colours by polarised light.

36. *Ivory.* A very thin film of ivory possesses neutral and depolarising axes, as perfectly as the most regularly crystallized mineral. When the vanished image is restored by the ivory, the intensity of the light of the other image is very much diminished, the difference between the two images being greater than I have found it in any other body. The film of ivory forms by transmitted light two highly coloured images

on each side of the common image, like those which I have described in my paper on mother of pearl.*

37. *Whalebone* depolarises light, and has neutral axes parallel and perpendicular to the direction of the fibres.

38. *Horn.* This substance also depolarises light in every position, and exhibits the coloured rings by polarised light.

39. *Mother of pearl* depolarises light in every position when the polarised pencil has a small angle of incidence; but when the angle of incidence is about 60°, and the plate about the thirtieth of an inch thick, it acts exactly like a bundle of glass plates, shifting merely the vanishing place of the image. †

40. *Bladder of a cow* depolarises light in every position.

41. *Human cornea* depolarises light in every position, and exhibits coloured rings by polarised light. The *tunica retina* and the *crystalline lens* exercise no peculiar action on light.

42. *Cornea of a cow* depolarises light in every position, and exhibits colours by polarised light. When it is pressed hard between the lenses, so as to induce a milky opacity, it still retains the power of depolarisation. The crystalline lens does not possess any of these properties.

43. *Cornea of a fish* depolarises light in every position, but most powerfully near its junction with the sclerotic coat, and exhibits coloured rings by polarised light. The crystalline lens, the sclerotic coat, and the capsule of the crystalline lens, exercise no action upon polarised light. When two capsules

* Phil. Trans. 1814. Part II. p. 397.

† We have here omitted the consideration of the nebulous image formed by mother of pearl at an oblique incidence. An account of the remarkable optical properties of this substance has been given in another paper.

were put together, a partial depolarisation took place, arising probably from the obliquity of the folds.

44. *Glue* depolarises light in every position.

45. *Hard isinglass* depolarises light in every position. When dissolved in water, it acquires this property a few hours after coagulation.

46. *Acetate of lead.* This salt melts at a temperature not much greater than that of bees' wax, and takes a long time to cool and crystallize. It depolarises light in every position. When the plates of glass, in which it is included, are considerably inclined to each other, the acetate of lead developes a second image in the act of cooling, but owing to the crystallization which takes place, both the images are imperfectly visible. Its refractive power increases a little after crystallization, and the new image that is developed is the one that is most refracted.

47. *Glass of borax.* A thick piece of this glass depolarises light in every position. Another piece of considerable thickness had no effect upon polarised light.

48. *Amber* sometimes depolarises light in every position, and sometimes exhibits neutral axes.

49. *Gum anime.* A piece of this gum, three quarters of an inch thick, depolarises light in every position, and seems to produce the complementary colours by polarised light. Small fragments of it depolarise only a small quantity of light.

50. *Sulphur*, when melted between two plates of glass, depolarises light in every position. It acquires this property in a few minutes.

51. *Ice.* Some plates of ice depolarise light in every position, while others exhibit neutral axes.

52. *Oil of mace.* This soft solid exhibits optical properties of a very peculiar character. When it is pressed into a thin film between two plates of glass without the aid of heat, it depolarises light in every position. It melts nearly at the temperature of blood heat, and takes a long time to cool and crystallize. If a thin plate of it is melted, and afterwards cooled between two pieces of glass, the image of a candle when seen through some parts of it, is encircled with a halo of nebulous light, varying in different plates from 0° to 16° in diameter, and having its central parts of a bluish colour, and the circumference of a reddish brown hue. In other parts of the film the halo disappears.

When the polarised light of a candle is transmitted through those parts of the film which do not produce the halo, it is depolarised in every position; but when the light is transmitted through the marginal parts, or those which produce the halo, the oil of mace restores *four wings of light* or *four luminous sectors,* in the centre of which is the place of the evanescent image. Through intermediate parts of the film, it depolarises two luminous images of the candle, separated by a narrow dark space, and manifestly formed of condensed nebulous light. Upon moving the film from the position which gives the luminous sectors, into that which gives the complete image of the candle, the wings or sectors gradually diverge from their common centre, and then vanish; and upon moving the film from the same position into that which gives the two luminous images, each adjacent pair of the sectors approach one another, and are condensed into two luminous semi-circles, which form the two images already mentioned. In the dark space between these two images, the

vanished image reappears by the slightest motion of the prism of calcareous spar. In some plates of oil of mace, this dark interval is occupied with a third image, so that the depolarised image has the appearance of being composed of *three images* closely pressing upon each other. In other plates the bright image is partly depolarised, even when the luminous sectors are visible.

The phenomena of the luminous sectors will be understood from Pl. V. fig. 1, where AB is the plane in which the light of the candle is polarised, and M N the two images of it formed by a prism of calcareous spar, *m* being the place of the vanished image, and *n* the visible image. The evanescent image at *m* is surrounded with the four luminous sectors 1, 3, 5, 7, separated by dark sectors 2, 4, 6, 8. The bright image of the candle at N is also surrounded with four luminous sectors 9, 11, 13, 15, separated by dark sectors 10, 12, 14, 16; but these sectors, of which 11 and 15 are the brightest, are not nearly so luminous as those at M.

If the oil of mace is kept in one position, while the prism of calcareous spar is turned round so as to make the image N move about M, as a centre in the direction BC, the evanescent image of the candle begins to appear at *m*: the luminous sectors turn round in the direction 1, 2, 3, 4, 5, 6, 7, 8: the sectors 1, 5 grow fainter, and 9, 15 brighter; and after the image N has moved through an arch of 45°, the two images M and N have nearly the same appearance. When the image N has described an arch of 90°, the sectors have the appearance represented in fig. 2, the candle having regained its full lustre in the middle of M, and having vanished in the middle of *n*. The sectors 1, 5 are now the faintest of those round

m, all of which are much inferior in brilliancy to those with which *n* is encircled.

When equal parts of *rosin* and *oil of mace* are mixed together, a film formed out of the mixture depolarises, imperfectly, the four luminous sectors.

53. *Tallow.* When tallow is melted between two plates of glass, and then slowly cooled, it exhibits no optical indications of a crystallized structure. After having stood five or six days, an incipient crystallization is exhibited in the property which it acquires of depolarising a small portion of nebulous light. This nebulosity gradually increases: about the eleventh day, it assumes an imperfect resemblance to the four luminous sectors produced by oil of mace; and about the sixteenth day, the form of the sectors is fully developed. Another plate formed of the fat of mutton, after standing five months, has acquired in some parts the property of depolarising a portion of nebulous light, while, in other parts, it depolarises a small part of the bright image. In a third plate, suddenly cooled by immersion in cold water, very faint traces of four large luminous sectors were visible by a careful exclusion of extraneous light.

54. *Tortoise shell* depolarises light in every position, and produces the coloured rings by polarised light. When a candle is viewed through tortoise shell, it is surrounded with a double halo, which becomes elliptical by inclining the plate to the incident rays. When the polarised light of a candle is depolarised, the restored image is surrounded with four very faint luminous sectors, like those in oil of mace.

55. *Heated glass.* When glass is brought nearly to a red heat, it depolarises light in every position, and the quantity

of depolarised light diminishes gradually with the temperature.

56. RUPERT'S *drops of unannealed glass.* When drops of melted glass are suddenly cooled by immersion in cold water, they acquire the faculty of depolarising light in every position, and of exhibiting the coloured rings by polarised light. At a certain thickness of the tail of the drop, the neutral axes are perfectly developed, and are parallel and perpendicular to the axis of the tail.*

57. *The semi-transparent and flat extremity of one of the legs of a young partan* gives only a nebulous image of a candle, but depolarises the nebulous light in every position.

58. A *tubular film* from the body of a partan, depolarises a small quantity of nebulous light.

The following table contains a list of substances chiefly of animal and vegetable origin, which have no effect in depolarising light.

Gold leaf.	Sclérotic coat of a fish.
Some crystals of diamond.	Crystalline lens of a fish.
Muriate of soda.	Crystalline lens of a cow.
Fluor spar.	Capsule of the crystalline lens of a fish.
A crystal of spinelle ruby.	Ambergris melted and cooled.
Muriate of ammonia.	Film which surrounds the hydatids.
Rochelle salts dissolved, and crystallized on the side of a glass.	Delicate film which lines the ribs of a lamb.
	Film from the stalk of the rhubarb.
Nitrate of lead dissolved, and crystallized on the side of a glass.	Film, or epidermis, which covers the shell *solen ensis*.

* A full account of the properties of heated and unannealed glass will be found in two former papers. (Phil. Trans. MDCCCXIV. Part II. p. 436, and MDCCCXV. Part I. p. 1.)

Resin of bile melted and cooled.
Jelly from calves' feet.
The skin of a fowl.
Scale from the body of a bee.
Hair of a bee.
Wing of a bee.
Wing of a house beetle.
Wing of the May fly.
Wing of the stone fly.
The byssus, or hair from the *pinna marina.*
Wing of the *meloë vesicatorius.*
Film which covers the tubular stalk of the *leontodon taraxacum.*
Film between the coats of an onion.
Film on the leaf of the American house leek.
Leaf of the hydrangea.
Spatha of a lily.
Film of gum Arabic formed by evaporation.
Rosin.
Gum copal.
Thin fragments of gum anime.
Gum galbanum.
Gum juniper.
Canada balsam indurated.
The spheres on sea-weed.
Film which lines the stalk of the *fleur de lys.*
Thin slices from a wafer.
Filaments of the pappus of the *leontodon taraxacum.*
Film which lines the shell of an egg.
Skin of a dried grape.
Phosphorus.
Hair from the fur of a seal.
Skin of an infant eleven months old.
Skin of a child two months before birth.
Skin of a herring.
Gum mastic.
Burgundy pitch.

II. *Theory of the depolarisation of light.*

The various modes in which bodies depolarise light may be reduced to *seven.*

1. When the crystal possesses neutral axes, and forms two images which are capable of being rendered visible, as in *calcareous spar, topaz,* &c.

2. When the crystal possesses neutral axes and exhibits only a single image, as the *human hair* and various *transparent films.*

3. When the crystal has no neutral axes, but depolarises light in every position, as in *gum Arabic, caoutchouc, tortoise shell,* &c.

4. When there is an approach to a neutral axis, as in *gold-beater's skin*, &c.

5. When the crystal depolarises, or restores only a part of the polarised image, as in a *film of sea-weed* and a *film* from the *partan*.

6. When the crystal depolarises luminous sectors of nebulous light, as the *oil of mace*.

7. When the crystal restores the vanished image, but allows it to vanish again during the revolution of the calcareous spar.

1. The *first* of these modes of depolarising light admits of an easy and satisfactory explanation.

Let R*r*, fig. 3, be the light of a candle completely polarised by reflection in the direction *r*S from the surface of the transparent body AB. If this light is viewed through a prism of calcareous spar CD,* when its principal section is neither coincident with, nor perpendicular to the plane of reflection R*r*S, two images of the candle will be seen; but upon turning round the prism CD, one of the two images will vanish alternately in every quadrant of the circular motion of the prism. Let the prism therefore be fixed in the position which it has when one of the images has vanished, in which case the principal section will be either parallel or perpendicular to the plane R*r*S. If a rhomb of calcareous spar MNOP is now interposed, so that the principal section MN is either parallel or perpendicular to the plane R*r*S, the vanished image will still be invisible. Upon turning round the rhomb MNOP, the

* Another prism is represented in the figure for the purpose of correcting, as much as possible, the refraction and dispersion of the prism of calcareous spar.

vanished image will begin to appear, and when MO is in the plane of reflection R*r*S, it will have reached its maximum brightness. It will again vanish when OP is in the plane of reflection, and will again recover its lustre when ON is in that plane, having vanished and reappeared *four* times in the course of one revolution of the rhomb. If the rhomb MNOP is kept fixed when the vanished image has reappeared; and if the prism CD is turned round, the two images will continue visible during every part of its circular motion, and hence the polarised ray *r*S, seems to have been robbed of its polarisation or depolarised.

In order to explain these appearances, let CD be fixed in its former position, and let the rhomb MNOP have its principal section or neutral axis in the plane R*r*S. This rhomb is known to give two images of the candle formed by rays ST, SV, nearly coincident, but owing to its present position, one of the pencils, that would have moved in the direction ST, refuses to penetrate the rhomb, and therefore only one pencil SV, polarised in the same manner as *r*S, falls upon the prism CD. Now this prism being obviously placed in the position where its power of doubling SV is extinguished, that is, where one of the pencils, into which it separates SV, has vanished, a single image E of the candle will still only be visible, notwithstanding the interposition of the rhomb. The very same reasoning is applicable to the case where the longer diagonal OP is in the plane of reflection.

The prism CD continuing fixed, as before, let the side MO, or the depolarising axis of the rhomb, be brought into the plane of reflection. In this situation of the crystals, both the pencils ST, SV, fall upon the prism CD, which has now the

particular position that enables it to double each of these pencils; so that *four* images of the candle 1, 2, 3, 4, will now be visible. As the rhomb MNOP produces only a very small separation of the pencils ST, SV, the two images 1, 2, will overlap each other, and resemble only one image at E, while the other two images 3, 4, will appear as a single image at F. Every thing remaining fixed, let the prism CD be turned round in a plane perpendicular to ST. The effect of this will be to extinguish one of each of the double images E and F at every quarter of a revolution, that is, first the images 1 and 3, then the images 2 and 4, then the images 1 and 3 again, and last of all the images 2 and 4. Still, however, one image is always left at E and another at F, so that when the polarised ray *r*S passes through the depolarising axis MO of the rhomb, the two images E and F continue visible in every part of the motion of the prism. The depolarisation, therefore, of the pencil *r*S, is nothing more than the polarisation of it in a new plane, and the depolarising rhomb MNOP acts in every respect like a doubly refracting and polarising crystal.

2. In the *second* kind of depolarisation where the *human hair*, or a *plate of mica* is substituted in place of the rhomb of calcareous spar, the phenomena are precisely the same as those which have been described in the preceding section, and therefore we are necessarily led to suppose that the human hair and the mica form two images polarised in an opposite manner, like those given by calcareous spar. These two images indeed being produced by the same, or nearly by the same refractive power, cannot be rendered visible by any contrivance; but when we consider that the depolarising axes of the mica coincide with the long and short diagonals of its

primitive rhomboidal base, as in the case of calcareous spar and topaz, and that there is a variation in the intensity of the light of the images E and F, during the revolution of the prism, we must consider the existence of two oppositely polarised images as no longer problematical.

Hence it follows, that every substance which possesses the property of depolarising light in the second manner, must necessarily form two coincident or nearly coincident images polarised in an opposite manner; or to speak more correctly, a pencil of common light transmitted through depolarising crystals, consists of a portion of light polarised like one of the pencils formed by calcareous spar, and of another equal portion polarised like the other pencil formed by calcareous spar.

Depolarising substances, consequently, are not entitled to the name of *doubly refracting crystals*, when the two oppositely polarised pencils are not capable of being separated from each other, and till this separation is actually *seen*, we must consider the two pencils as produced by the same refractive power.

We would therefore propose to designate all substances that form two separable images, such as calcareous spar, quartz, topaz, &c. by the name of *doubly refracting crystals*, and those which do not form two separable images, such as diamond, mica, heated glass, the human hair, &c. by the name of *doubly polarising crystals*.

3. The *third species* of depolarisation is characterised by the substance having no neutral axes, and depolarising light in every position; and is possessed by *gum Arabic*, *caoutchouc*, and many other bodies which are known to be formed by the successive deposition and induration of thin layers.

When the first layer of gum Arabic or caoutchouc is deposited and crystallized, it will possess both neutral and depolarising axes like every other crystal. The second layer will likewise have these axes, but there is manifestly no cause which can make the neutral and depolarising axes of the second layer coincide with those of the first layer; so that after a number of layers are formed, there will be a depolarising axis in every direction. In order to illustrate this conclusion by direct experiment, we have only to place one plate of mica above another, so as to make the neutral axis of the one coincide with the depolarising axis of the other. It will then be seen that all the axes become depolarising axes, and that the compound crystal acts upon light exactly like gum Arabic and caoutchouc. If this explanation be correct, we should expect to find, that a film of gum Arabic or caoutchouc, reduced to a less thickness than any individual layer, would exhibit its neutral axes, and lose the property of depolarising light in every position. This interesting result I have repeatedly obtained both with gum Arabic and caoutchouc, as described in experiments 1 and 3, so that we have both a synthetical and an analytical proof of the explanation which has been given of the *third* kind of depolarisation.

Hence it follows, that all substances which depolarise light in every position, are formed by layers successively deposited and crystallized; that every layer has neutral and depolarising axes, like regularly crystallized bodies; and that the axes of one layer is not related in point of direction to those of its adjacent layers.

4. The *fourth* kind of depolarisation is exhibited in the film of gold beaters' skin, where there is an approach to a neutral axis.

When a body is composed of two or more films whose neutral axes are nearly coincident, the compound film will exhibit the fourth kind of depolarisation, and the approximation to a neutral axis will be more or less perfect, as the coincidence of the axes is more or less complete. This phenomenon is most likely to be exhibited by thin plates which are composed of a small number of films.

Hence every body, in which there is an approach to a neutral axis, must be composed of two or more films, whose neutral axes happen to be nearly coincident. These films are probably deposited and crystallized at different times; or if their crystallization has been simultaneous, the forces, or causes by which it was produced, must have acted independently of each other.

5. The *fifth* kind of depolarisation takes place when there is an approach to a depolarising axis, or when the crystal restores only a portion of the vanished image.

If we suppose one part of a body to have no crystalline texture, while another part of it has the structure necessary to depolarise light, it will exhibit correctly the *fifth* kind of depolarisation. The uncrystallized portion being incapable of restoring any part of the vanished image, the light which it transmits will form no part of the depolarised pencil, which will consist merely of the rays transmitted through the structure which has the property of *double polarisation*. The magnitude, therefore, of the depolarised pencil will be a measure of the portion of the substance which has undergone crystallization. From the phenomena of caoutchouc, this explanation derives great support. When the crystalline texture of this substance has been destroyed by heat, it ceases to act upon

polarised light. After it begins to crytallize, however, a *small portion* of light is at first depolarised. This portion gradually increases, and the image is not completely restored till the crystallization has pervaded the whole mass. The experiments with oil of mace furnish us with another proof of this explanation, and exhibit a case in which a part of the substance is permanently crystallized, while another part of it is permanently uncrystallized.

Hence we may conclude, *that bodies which depolarise only a portion of light, either consist of permanently crystallized and uncrystallized portions, or are in a state of approach to a perfect crystalline structure, the crystallized portion being always proportional to the quantity of depolarised light.*

6. The *sixth* kind of depolarisation is exhibited in the curious phenomena of *oil of mace*, which sometimes depolarises *four* sectors of nebulous light.

Those plates of oil of mace, in which the luminous sectors are alone depolarised, have obviously two structures, namely, that which forms the bright, and that which forms the nebulous image. The structure which forms the bright image has no more action upon light than a mass of water, as it does not in the slightest degree alter its polarity; but the structure which forms the luminous halo possesses a peculiar character. If we suppose the halo to be divided, as in Pl. V, fig. 1; into eight sectors 1, 2, 3, 4, 5, 6, 7, 8, every alternate sector 1, 3, 5, 7, is polarised in the same manner as the incident light, while the other sectors 2, 4, 6, 8, are polarised in an opposite manner. Now, if this halo consisted only of one nebulous image, the evanescence of every alternate sector would take place without applying the calcareous spar, and merely by

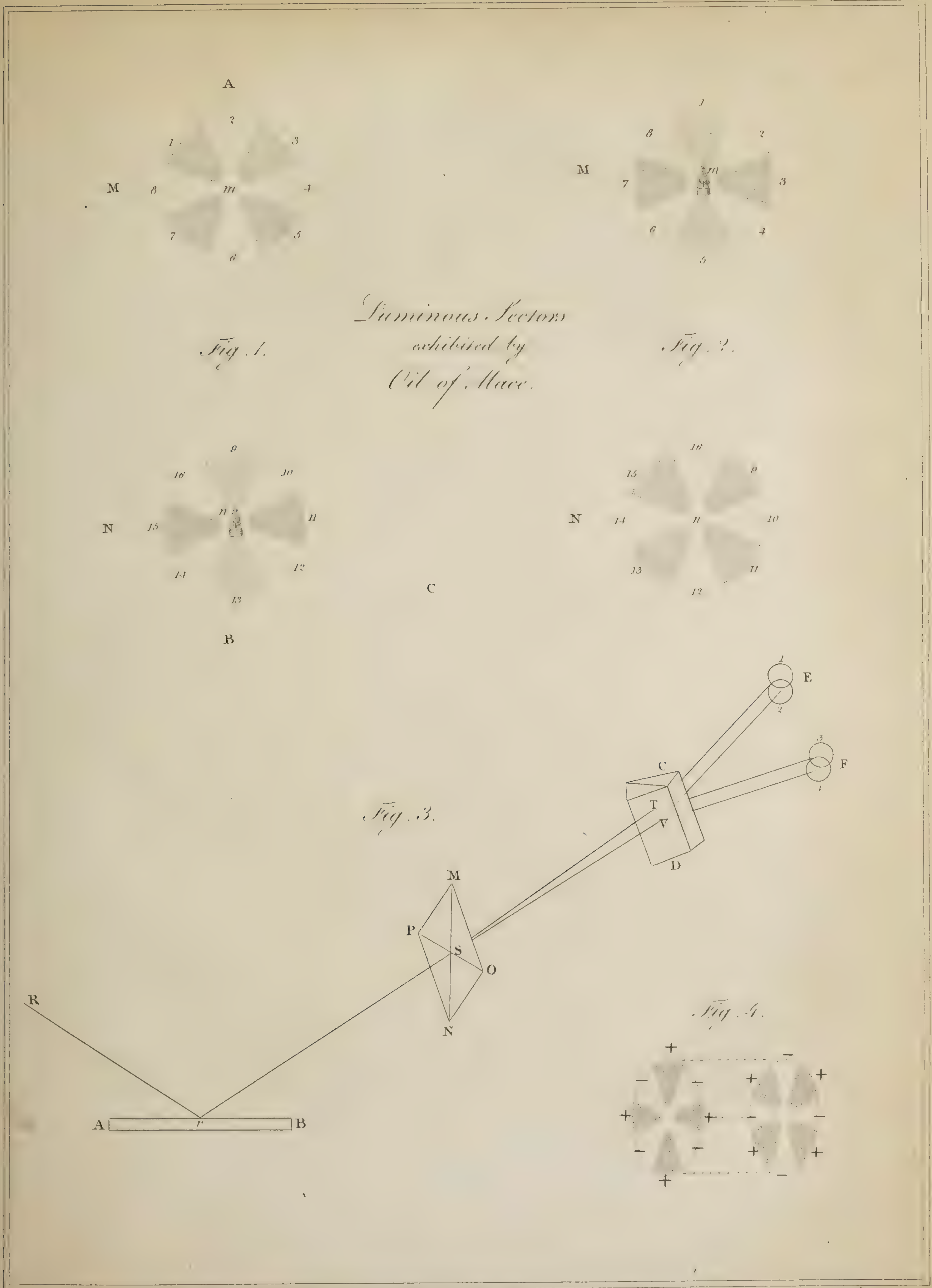
transmitting polarised light through the oil of mace; but as this is not the case,* it necessarily follows that there are *two* halos, or nebulous images, the one lying exactly above the other, and having every alternate sector polarised in an opposite manner, while each sector in the one image has an opposite polarisation to the corresponding sector in the other image. An idea of this curious property may be formed from fig. 4, in which we have shown the two halos at a distance, and distinguished the opposite kinds of polarisation by the signs + and −.

Those parts of oil of mace which depolarise a portion of the bright image, while they form the luminous sectors, have therefore the faculty of forming *four images*, two bright and two nebulous, possessing the characters which have already been described.

The two adjacent images which are formed by some portions of the plate of oil of mace, are obviously produced by the sectors 1, 7, and 2, 5, fig. 1 being condensed on each side of *m*, and when three images are depolarised, the third image is a portion of the bright image restored at *m*, the place of its evanescence.

If we knew in what way the halo itself is formed, there would probably be no difficulty in explaining these remarkable phenomena. The diameter of the halo is too small to allow us to suppose that the polarisation of the sectors can be effected by oblique reflection or refraction, and though it is extremely probable that light is partially polarised by inflexion

* In some instances, when we examine the halo formed by polarised light without applying the calcareous spar, the two sectors in the plane of polarisation are less luminous than the rest.


Luminous Sectors
exhibited by
Oil of Mace.
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
A
B
C
M
N
m
n
1
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3
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and deflexion, yet we are not entitled to employ this conjecture in the explanation of phenomena.

7. In all the preceding cases of depolarisation, the depolarised image continues visible in every part of the circular motion of the prism of calcareous spar, but there are cases where the vanished image is restored, and again vanishes during the revolution of the prism.

This phenomenon takes place when the polarised pencil is depolarised by transmitting it along the short diagonal of a rhomb of calcareous spar, or along the axis of a hexaedral prism of nitre, or through a parcel of glass plates, or through plates of agate and carbonate of barytes, that give a bright and a nebulous image. In all these cases only one bright image is produced, so that the images must vanish alternately in every quarter of a revolution, the only effect of the depolarising body being to polarise the light in a different plane, and thus to shift the vanishing place of the images.

Hence it follows that every body which possesses this kind of depolarisation, forms either a bright and a nebulous image, like the agate, or a single image, the light of which is all polarised in the same manner.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, October 22, 1814.

IV. *On an ebbing and flowing stream discovered by boring in the harbour of Bridlington. By* John Storer, *M. D. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read January 19, 1815.

THE following account of certain peculiarities attending a spring of fresh water, which was tapped in boring within the harbour of Bridlington quay, Yorkshire, is given from repeated observations made during a residence of some weeks there, in the months of July and August, 1814. The harbour of Bridlington quay is dry at low water, except for a rivulet which traverses its bed: at high water, it has from fifteen to seventeen feet of water.

Mr. Rennie, civil engineer, was consulted in the year 1811, respecting certain improvements projected in that harbour. At his desire, with a view to ascertain the depth of the stratum of clay in the harbour, the boring, which terminated in forming the well to be described, was begun under the direction of Mr. Milne, collector of the customs for the port. The spot fixed upon is opposite to the termination of a street leading to the harbour, and has about six feet of water, at high water, in ordinary tides.

After the workmen had bored through twenty-eight feet of very solid clay, and afterwards through fifteen feet of a cretaceous flinty gravel, of a very concrete texture, the auger was perceived to strike against the solid rock; but as they were not

able to make any impression upon it, the work was given up for that tide, without any appearance of water from the first. In an hour or two afterwards, the bore was found filled to the top with fresh water, of the most limpid appearance: it soon flowed over, and was even projected some inches above the summit of the bore, in a stream equal to its calibre. When it was ascertained that the water was of the purest quality and taste, perfectly fit for washing, and every culinary purpose, the bore was properly secured by an elm stock, ten feet long, and perforated with a three inch auger, driven to its full length: a copper tube, well tinned on both sides, of a circumference to admit its being passed through the bore of elm stock, and thirty-two feet in length, was then forced to the bottom of the bore, so as to rest on the rock. The upper part being properly puddled round the elm stock, and the well thus completed, the following singular circumstances were observed, and have continued with great uniformity ever since.

As soon as the surface of the sea water in the harbour, during the flowing tide, has arrived at a level of forty-nine or fifty inches lower than the top of the bore, the water begins to flow from it, in a stream equal to its calibre, the impetus of which is increased as the tide advances, and may be observed to be propelled with much force after the bore is overflowed by the tide. The discharge continues from four to five hours, *i. e.* till the tide in returning falls to the same level where it began to flow: at this point, it ceases completely till the next flood shall have regained the same level, when the same phenomena recur, in the same succession; and without any variation, but what arises from the different degrees of elevation in the tides. The rule appears to be, that the

column of spring water in the bore, is always supported at a height of forty-nine or fifty inches above the level of the tide, at any given time. This at least was the result of every observation I made during several successive weeks, in the months of July and August last; and I am assured by Mr. Milne, on whose ingenuity and habit of accurate observation I can place the firmest reliance, that his habitual experience, for three years past, goes to convince him, that the variations from the rule stated above, are very inconsiderable during the summer and autumnal months; but that in winter, after any unusual fall of rain, he has known the column of fresh water raised eight feet above the level of the tide, and the period of its discharge proportionally prolonged.

For the use of the town and shipping, a reservoir of brick-work, capable of containing one thousand gallons, has been constructed within two or three yards, and upon somewhat a higher level than the summit of the bore, and is made to communicate with it by a tube of the same diameter, fitted with a valve, to prevent any reflux into the well. Two waste pipes are placed within a foot of the top of the reservoir, for the regular discharge of the water, and it has also been made to communicate with a pump adjoining, by which the reservoir may be emptied; and as the bore of the well is now closed and secured at the top, it is obvious that the commencement of the flow of water, from the pipes of the reservoir, will happen a few minutes sooner or later at each tide, according to the quantity of water it contained at the time. Such, however, is the known regularity of the discharge from the waste pipes, that at the expected time of the tide several of the inhabitants are always on the spot with

their vessels, and are rarely obliged to wait for more than five minutes.

Such is the state of facts, and it appears to open a subject of curious investigation to those whose habits and practical knowledge qualify them for it. The appearances seem not to admit of any satisfactory explanation, without supposing some mode of subterranean communication, by which the water of the sea, and that of the spring in question, are brought into actual contact, so as to exert a reciprocal action. This supposition receives considerable support from a circumstance which I had no opportunity to observe, but which Mr. MILNE has had frequent occasion to notice; and which he describes by remarking, that after stormy weather, when there is a heavy sea on that coast, the water is discharged, even from the waste pipes of the reservoir, with an evident undulation; which, of course, would be more considerable from the original bore.

Mr. MILNE has framed an hypothesis to satisfy his own mind on this curious subject. He believes the stratum of clay found in the harbour, to extend over the whole bay in front of it, as far as the Smithwick sand, which forms a bar across the opening of the bay, in a direction from Flamborough head towards the Spurn point, and about four miles from the quay in a south-easterly direction. This bank is supported by a reef of rock; and though there are openings, which are well known, and admit vessels of considerable burden at all times of the tide, there is in general but a small draft of water on this bank, when the tide is out. On the outward or east side, towards the ocean, the rock is quite perpendicular, and a great depth of water is immediately behind it. As the copious source

of water, which has been tapped in the harbour, lies at such a depth, and under a stratum of clay, there is no reason to think that it can be discharged any where in the bay, till it arrives at the ledge of rock where the clay terminates. Here, among the fissures of the rock, it may find its exit; and this is the more likely, as it is known that the bed of the sea at the back of the Smithwick sand, is at so much a lower level.

Admitting this supposition to be correct, or nearly so, it seems to follow, that the issue of a body of fresh water, through a fissure in rock forming the bed of the sea, would meet with more or less resistance at different times of the tide; because the two columns of fluid in meeting, would act upon one another in the ratio of the altitude of each, taking into the account the difference of their specific gravity; and thus, if there is any approach to an equilibrium, an operation would result, analogous to the flux and reflux of the tide, near the mouth of rivers.

This hypothesis is specious, and accounts for the flux and reflux of the water from the bore, as well as for the singular undulation of the discharge in a boisterous state of the sea: but the greater relative altitude to which the column of spring water is elevated after much rain, and the consequent prolonged discharge of it during each tide, seems to militate against its correctness; since, in a case, where by the supposition a balance is nearly established, an additional impetus communicated to the column of spring water, ought to produce the opposite effect, by enabling it to overcome the resistance of the same column of sea water during a longer period of each tide, than under the usual circumstances.

It is not improbable, that this whole subject might be

elucidated, by a more perfect acquaintance with the peculiarities of the springs on this part of the coast, provincially termed *gipsies*. The water in this district of the east riding of Yorkshire, possesses that limpidness which is usual in cretaceous soils; but for many miles of the Wolds behind Bridlington, very little water is to be seen. There are few rivulets, and these are very low in the summer, and most of them quite dry in autumn. The account to be collected from the inhabitants is, that in two or three weeks after the commencement of frost, the springs begin to run copiously; and in many, the water is projected with such impetuosity, as to resemble a *jet d'eau*; it is then that, in the language of the country, it is said, " the gipsies are up," and the rivulets overflow.

JOHN STORER.

Nottingham, Nov. 5, 1814.

Drawn up from Notes taken at the time, and on the spot.

V. *On the effects of simple pressure in producing that species of crystallization which forms two oppositely polarised images, and exhibits the complementary colours by polarised light. By* David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.*

Read January 19, 1815.

Dear Sir,

In prosecuting the experiments on the depolarisation of light, which you lately did me the honour to lay before the Royal Society, I have been led to the discovery of a remarkable property of soft transparent solids, in virtue of which they exhibit, by simple pressure, all the optical qualities of doubly polarising crystals. In the paper on depolarisation to which I have now alluded, it has been shown that a mixture of bees' wax and rosin, when melted and cooled between two plates of glass, depolarises a ray which falls upon it at a vertical incidence, while the same substance, pressed between two plates of glass, without the aid of heat, produces no effect when the polarised ray falls perpendicularly upon it, but depolarises it at an oblique incidence. In this experiment the crystallization was not produced by pressure, as the unmelted bees' wax was already crystallized; but it is obvious, either that the pressure had modified the natural crystallization of the bees' wax, so as to enable it to depolarise only at an oblique incidence, or that its liquefaction between two plates of glass had

produced such a change, as to communicate to it the property of perpendicular depolarisation.

In whatever manner this difference of action was produced, the effects of pressure seemed to require farther investigation, and in order to be able to apply a sufficient force, without injuring the structure of the substance, I employed animal jellies which could be brought to any degree of tenacity without losing their transparency.

Having cut out of newly made calves' feet jelly, a cylindrical portion, about half an inch broad and half an inch high, I placed it between two plates of glass, and observed that it did not possess, in the slightest degree, the property of depolarising light. After standing some days, it began to depolarise light at its circumference, and in the course of fifteen days this property gradually extended to its central parts. The cylinder of jelly had at first such a slight degree of tenacity, that it quivered with the gentlest motion; it was now however considerably indurated, and though it formed a plate exactly parallel, yet it diverged the incident rays like a concave lens, from the external parts having a greater degree of induration, and consequently a higher refractive power than the parts towards the centre. At the end of three weeks it began to lose its transparency, and at the same time its depolarising structure; and in the course of a few days more, it had no more action upon light than a mass of water. Its thickness was now reduced, by contraction, to about one-seventh of an inch, and it possessed a degree of tenacity, approaching to that of caoutchouc, which enabled it to sustain, without injury, a very considerable degree of pressure.

In this state, I exposed the plate of jelly to the light of a

candle polarised by reflection, and employing a prism of Iceland spar, one of the images of the candle vanished at every quadrant of its circular motion, just as if the jelly had not been interposed. I now pressed together the two plates of glass, that inclosed the cake of jelly, and was surprised to find that the vanished image was restored, the light being depolarised in every position of the cake. Upon removing the pressure, the image again vanished, and the cake resumed its uncrystallized state.

In order to vary the experiment, I took two prisms of Iceland spar, and having put them in such a position that two of the four images of a candle vanished; I then placed the cake of jelly between the prisms, and pressing them hard together, the two vanished images were restored, the depolarisation being more perfect as the pressure was increased. The removal of the pressure again destroyed the depolarising structure.

I repeated the preceding experiments with another plate of jelly, which was perfectly transparent, and which possessed the depolarising structure only at the edges. The pressure never failed to communicate to the central parts the property of depolarisation, and I repeatedly observed that the complementary colours produced by topaz, &c.* were imperfectly exhibited when the pressure had attained a particular magnitude.

As the cakes of jelly used in the preceding experiments, had both been crystallized by induration, I took another cylindrical portion that had never possessed that crystalline structure which is necessary to depolarise light. It was about an inch high, and three quarters of an inch broad, and was so

* See Phil. Trans. 1814. Part. I.

extremely soft that it could scarcely support itself in a vertical position. Having exposed it to a polarised ray, as before, I pressed its cylindrical circumference between my finger and thumb, and thus enabled it to restore the vanished image. A slight pressure, indeed, from one finger, was capable of producing a perceptible degree of depolarisation.

Instead of calves' feet jelly, I next employed *isinglass*, brought nearly to the consistency of caoutchouc. After standing a day, the isinglass had, of its own accord, acquired the depolarising structure, even when cut into very thin films, either parallel or perpendicular to the surface; but upon placing a cake of it, about a quarter of an inch thick, between two plates of glass, and exposing it to polarised light, I found that the complementary colours were developed in a most beautiful manner by hard pressure, and I often saw a portion of a red and a blue ring upon one of the images of the candle, while the colours complementary to these occupied the other image. By varying the pressure new colours arose, and when the pressure was removed, the complementary tints gradually disappeared. As these changes of colour might be ascribed to the pressure, only in so far as it reduced the cake of isinglass to the degree of thickness necessary for their production, I brought the cake to the same thickness which it possessed when exposed to the pressure that developed the most lively colours. No colours, however, were now visible, but they were instantly reproduced, as before, by the application of pressure.

I now melted the isinglass between two plates of glass, and allowed it to stand till it coagulated, which took place in less than a quarter of an hour. Upon transmitting through it a

polarised ray, I saw that it did not in the least degree depolarise it. I then exposed the included jelly to a considerable pressure, and it instantly restored the evanescent image, and exhibited, in a faint degree, the complementary colours. This plate was not more than $\frac{1}{20}$th of an inch thick.

From these experiments and others, which have been repeated under various modifications, it follows:

1st. *That soft animal substances which have no particular action upon light acquire, from simple pressure, that peculiar structure which enables them to form two images polarised in an opposite manner, like those produced by all doubly refracting crystals, and to exhibit the complementary colours produced by regularly crystallized minerals.*

2d *That soft animal substances, which already possess the property of depolarising light, receive from simple pressure such a modification in their structure as to enable them to exhibit, in a very brilliant manner, the complementary colours produced by crystallized minerals.*

3d. *That soft animal substances which only depolarise a portion of the incident ray, have their depolarising structure completed by simple pressure.*

The extension of these experiments to other soft substances, to hard bodies when in a fluid state, and to fluids themselves, may probably lead to still more interesting results.

I have the honour to be, Dear Sir,

your most obedient humble servant,

DAVID BREWSTER.

Edinburgh, January 3, 1815.

To the Right Hon. Sir JOSEPH BANKS, Bart. K. B. P. R. S.

VI. *Experiments made with a view to ascertain the principle on which the action of the heart depends, and the relation which subsists between that organ and the nervous system. By* A. P. Wilson Philip, *Physician in Worcester. Communicated by* Andrew Knight, *Esq. F. R. S.*

Read February 9, 1815.

The following experiments were begun with a view to ascertain the manner in which certain poisons act in destroying life. I soon found that, in order to make any considerable progress in such an inquiry, it is necessary to ascertain how far the powers of the nervous and sanguiferous systems directly depend on each other. There seems never to have been any difference of opinion respecting the direct dependence of the nervous on the sanguiferous system. When the powers of circulation are increased or diminished, the nervous system always suffers a corresponding change, nor can the latter, under any circumstances, continue to perform its functions after the former are destroyed. I speak of the warm blooded animals. In cold blooded animals the process of dying is so slow, that the functions of the nervous system abate very gradually, after the circulation has wholly ceased. The converse of the above proposition is by no means so generally admitted. It is evident that certain changes of the nervous, produce corresponding changes in the sanguiferous, system; yet, while some assert, that the action of the heart

depends as immediately on the brain, as that of the latter does on the heart, others maintain, that the nervous power may be wholly destroyed without impairing the vigour of this organ. This point it is necessary to determine, before we can trace with precision the *modus operandi* of poisons. The following inquiry therefore may be divided into two parts. In the first, I shall endeavour to ascertain how far the power of the heart is influenced by the state of the nervous system; in the other, by what steps certain poisons destroy the powers of both. This I shall reserve for another paper, and here confine myself to the first part of the subject.

Till the time of HALLER, it seems to have been the general opinion, that the muscles derive their power from the nervous system. He taught, that the power of the muscles depends on their mechanism, that the nervous influence is merely a stimulus which calls it into action, and consequently that those muscles, the heart for example, which act only by the application of one peculiar stimulus, unconnected with the nervous system, are wholly independent of it. This opinion seemed confirmed by its being generally admitted, that the action of the heart continues after it is removed from the body, and that it cannot be influenced by stimulating the brain, or spinal marrow, or the nerves which terminate in it. HALLER and his followers maintain, that there are two distinct vital powers, one of the nervous and another of the sanguiferous system.

The supporters of HALLER's doctrine however, found many difficulties to contend with. The evident objections to it are, that the heart is influenced by affections of the mind, and that it is supplied with nerves. Various hypotheses have been

framed to get rid of these objections, some of which imply a considerable modification of the original opinion. Several writers have maintained, that although the heart is independent of the brain and spinal marrow, it may be subject to some peculiar action of its own nerves; others, that the ganglia through which its nerves pass have a power independent of the sensorium commune. FONTANA and others have maintained, that the nerves of the heart are absolutely useless; others, that these nerves are distributed on its vessels, and do not enter the substance of the heart. SCARPA, however, has proved, that nerves are distributed to the heart in the same way as to other similar parts. Nothing can show more strikingly the imperfection of our knowledge of this important branch of physiology, than that opinions so different, and so destitute of proof, should be maintained by the best writers upon it.

An author has lately appeared, who, among other ingenious and important experiments, has made many relating to this subject, and arrived at conclusions which have surprised physiologists, yet apparently so well supported as to have obtained their general assent. M. LE GALLOIS* maintains, that by the destruction of the whole or cervical part of the spinal marrow, the action of the heart is immediately so debilitated, that it is no longer capable of supporting the circulation; while by the destruction of the brain, on the contrary, its action is unimpaired: from which he infers, that it is from the

* Expériences sur la principe de la Vie, notamment sur celui des mouvemens du cœur, et sur le siège de ce principe, suivies du Rapport fait à la premiere Classe de l'Institut, sur celles rélatives aux mouvemens du cœur, par M. LE GALLOIS, Doct. en Méd. &c. Paris, 1812.

spinal marrow that the heart derives the principle of its life and of its motions. Those motions of the heart, says M. le Gallois, which remain after the destruction of the spinal marrow, or the interruption of the nervous influence upon the heart in any other way, and which misled Haller and his followers, are motions without force, incapable of supporting the circulation, and analogous to the motions of other irritable parts on the application of a stimulus, which in this case is the arterial blood contained in it.

The experiments, on which these opinions are founded, he repeated in the presence of a Committee of the National Institute at Paris, which has expressed its conviction of their accuracy. Notwithstanding this high authority, I was led, from some experiments which I made many years ago, in which both the brain and spinal marrow were destroyed by the action of opium and tobacco, to doubt M. le Gallois' conclusions. The reader will judge how far the following experiments tend to invalidate these conclusions, and influence our opinions of the subject to which they relate.

I cannot here omit to express my thanks to Mr. Hastings, House-surgeon to the Worcester Infirmary, who assisted me in the following experiments. His expertness in dissection was often of great use, where it was necessary to be expeditious, and to lose as little blood as possible.

Exp. 1. A rabbit was deprived of sensation and voluntary power by a stroke on the occiput. When the rabbit is killed in this way, the respiration immediately ceases; but the action of the heart and the circulation continue, and may be supported for a considerable length of time by artificial respiration, as practised first, I believe, by Fontana, and since by

Chirac, Mr. Brodie, M. le Gallois, and others. This mode of destroying the sensibility does not influence the result of the experiment, and has the double advantage of preventing the animal's sufferings, and his motions. Its greatest inconvenience is, that if the blow is very severe, considerable vessels are sometimes ruptured, and there is always some rupture of vessels, which of course tends to impair the vigour of the circulation.

In the present experiment, the circulation was supported by artificial respiration. The spinal marrow was laid bare from the occiput to the beginning of the dorsal vertebræ. The chest was then opened, and the heart found beating regularly, and with considerable force. The spinal marrow, as far as it had been laid bare, was now wholly removed, but without in the least affecting the action of the heart. After this, the artificial respiration being frequently discontinued, we repeatedly saw the action of the heart become languid, and increase on renewing it. The skull was then opened, and the whole of the brain removed, so that no part of the nervous system remained above the dorsal vertebræ, but without any abatement of the action of the heart, which still continued to be more or less powerful, according as we discontinued or renewed artificial respiration. This being for a considerable time discontinued, the ventricles ceased to beat about half an hour after the removal of the brain. On renewing the respiration, however, the action of the ventricles was restored. The respiration was again discontinued and renewed, with the same effects.

Exp. 2. A rabbit was made insensible by removing part of the skull, and applying opium to the brain. The spine was

then opened between the cervical and dorsal vertebræ. We then laid open the thorax, and supported the action of the heart by artificial respiration. The force with which it beat was carefully observed, and the spinal marrow destroyed by running a hot wire up and down the spine, through the opening made in it, by which the action of the heart was not at all affected.

Exp. 3. In the foregoing experiments, it may be said, there was no direct proof of the continuance of the circulation after the spinal marrow was destroyed or removed. On this account several of the following experiments were made. A rabbit, previously exhausted by dividing the eighth pair of nerves, was deprived of sensation by a blow on the occiput, and the circulation supported by artificial breathing. The carotids were seen beating near to the place where the nerves had been divided. The cervical part of the spinal marrow was then destroyed by a hot wire, after which the carotids were still found beating.

Exp. 4. In a rabbit rendered insensible by a blow on the occiput, the whole spinal marrow was destroyed by a hot wire, and the breathing artificially supported. One of the carotid arteries was then laid bare. Its beating was evident, and on dividing it, florid blood flowed from it freely.

Exp. 5. The only difference between this and the last experiment was, that artificial breathing was not performed. In both, the spinal marrow was destroyed, by introducing a wire hot enough to make a hissing noise through an opening between the cervical and dorsal vertebræ, first through the upper portion into the brain, then through the under portion to the end of the spine. On laying open one side of the neck, the

carotid artery was found beating. On dividing it, blood of a much darker colour than in the former experiment was thrown out copiously *per saltum.*

Exp. 6. A rabbit was rendered insensible by a blow on the occiput, and artificial respiration maintained. The spinal marrow from the base of the skull to the beginning of the dorsal vertebræ was removed, and a hot wire forced through the remaining part of the spine. The carotid artery was then found beating, and on dividing it florid blood rushed out with great force *per saltum.*

Exp. 7. This experiment resembled the last, except that the spinal marrow, instead of being partly removed, was wholly destroyed by a hot wire, and artificial breathing was not performed previous to opening the carotid, from which dark coloured blood flowed *per saltum.* We then inflated the lungs, and arterial blood soon began to flow copiously from the vessel, and appeared like a florid stream mixing with the dark coloured blood which had previously come from it. This experiment was repeated in the same manner, and with the same result.

Exp. 8. In this experiment the rabbit was rendered insensible, but not motionless, by the blow on the occiput, so that the breathing still continued. The spine was opened, and the spinal marrow destroyed, as in the preceding experiment. The wire was used very hot. On introducing it through the spine into the brain, the breathing immediately ceased. The femoral artery was laid bare about two or three minutes after respiration had ceased. The beating of the artery was evident. On opening it, a dark coloured blood flowed from it freely. We now had recourse to artificial respiration. When

it had been continued for about half a minute, the blood, which continued to flow copiously from the artery, became of a highly florid colour. The other femoral artery was then opened, from which florid blood also flowed freely. When about an ounce of blood had flowed from the two vessels, the inflation of the lungs was discontinued, and the blood again flowed of a dark colour. On renewing the inflation of the lungs, the blood, in less than half a minute, again became of a florid colour. It continued to flow from the femoral arteries altogether for seven minutes. Three minutes after the blood had ceased to flow from them, the artificial respiration being continued, one of the carotid arteries was opened, from which a florid blood flowed in a free stream, to the amount of a dram and a half. The flow from the carotid artery ceased in eleven minutes after the femoral artery had been opened. Most of the blood was now of course evacuated. A good deal had been lost in opening the spine, which always happens. The left auricle and ventricle were found nearly empty. The blood which remained in them was florid. The right auricle and ventricle were full of dark blood.

Exp. 9. From various trials, we found that in such experiments the circulation ceases quite as soon without, as with the destruction of the spinal marrow. Loss of blood seems to be the chief cause which destroys it. When the animal was operated upon, without being rendered insensible, pain also contributed to this effect. We frequently, after laying open the skull and spine, found the circulation lost before either the brain or spinal marrow had been disturbed. In the younger rabbit, it was lost sooner than in the older. The former seemed to die sooner from any injury, except the inter-

ruption of respiration. The circulation is particularly apt to fail, if artificial respiration is not carefully performed after the animal ceases to breathe. In making such experiments, after opening the bone, it is always necessary to ascertain whether the circulation continues, before we destroy or remove the brain or spinal marrow. As little blood is lost in this part of the operation, when the carotid arteries were beating before, we always found them beating after it. The result of this experiment is still more striking in the cold blooded animals, in which death takes place so slowly, that the circulation continues long after the total destruction of the nervous system.

Exp. 10. The brain of a frog and the spinal marrow as low as the dorsal vertebræ were laid bare. The thorax was then opened, and the heart found acting vigorously; and from the transparency of its sides, the passage of the blood through it distinctly seen. The part of the spinal marrow, which had been laid bare, was then removed, but without at all affecting either the motion of the heart, or the passage of the blood through it. The brain was then removed, with the same result.

Exp. 11. The brain and spinal marrow of a frog were wholly removed. On opening the thorax, the heart was found performing the circulation freely.

I have already had occasion to observe, that it is generally admitted that the action of the heart cannot be influenced by stimuli applied to the nervous system: and it seems almost a contradiction to suppose that it should, when we see that it cannot be influenced by the total destruction of this system. There were many reasons, however, which induced me to try

the effect on the heart of stimuli so applied to the brain and spinal marrow, as not to excite any of the muscles of voluntary motion, whose action, either by throwing more blood towards the heart, or in some other way influencing its action, prevents our judging of the effect of the stimulus.

Exp. 12. A rabbit was deprived of sensation and voluntary motion by a blow on the occiput, the action of the heart supported by artificial respiration, and the brain and cervical part of the spinal marrow laid bare. The thorax was now opened, and the action of the heart, which beat with strength and regularity, observed. Spirit of wine was then applied to the spinal marrow, and a greatly increased action of the heart was the consequence. It was afterwards applied to the brain with the same effect. The increase of motion was immediate and decided in both cases. We could not perceive that it was more in the one case than the other.

Exp. 13. The foregoing experiment was repeated, with the difference, that the whole of the spinal marrow was laid bare. The motion of the heart was nearly, if not quite, as much influenced by the application of the stimulus to the dorsal, as to the cervical portion of the spinal marrow; but it was very little influenced by its application to the lumbar portion.

Exp. 14. In this experiment, only that part of the brain which occupies the anterior part of the head was laid bare. The rabbit in other respects was prepared in the same way as in the preceding experiments. The spirit of wine applied to this part of the brain, produced as decided an effect on the motion of the heart as in those experiments. The spirit of wine was washed off, and a watery solution, first of opium, then of tobacco, applied, with the effect of an increase, but a

much less increase of the heart's action than arose from the spirit of wine. The increased action was greater from the opium, than from the tobacco. The first effect of both was soon succeeded by a more languid action of the heart than that which preceded their application to the brain. This effect was greatest, and came on soonest when the tobacco was used, and we always, for we frequently repeated the experiment, saw an evident increase in the action of the heart, when we washed off the tobacco. We could also perceive this, though in a less degree, when the opium was washed off. Little or none of this debilitating effect was observed when the spirit of wine was used. After its stimulating effect had subsided, the action of the heart only returned to about the same degree as before the application of the stimulus.

Exp. 15. The foregoing experiment was repeated on an animal of cold blood. Mr. HASTINGS had found, that immersing the hind legs of a frog in tincture of opium, in less than a minute, deprives it of sensibility. This does not arise from any action of the opium; a watery solution of opium, we found, however strong, does not produce the effect. It is immediately produced by simple spirit of wine, and arises from the action of the spirit on the nerves of the part to which it is applied, for it takes place quite as readily as in the healthy frog, after a ligature has been thrown round all the vessels attached to the heart. It is remarkable, that if simple spirit of wine is used, the animal expresses severe pain, if tincture of opium, very little. I have already mentioned the reason why it is necessary, in order to judge of the result of this experiment, that the animal should be rendered insensible. (*Exp.* 11.)

Having thus deprived a frog of sensibility, we laid bare the

brain and spinal marrow, and opened the chest. The heart was found contracting with vigour. Spirit of wine was then applied to the spinal marrow, with an immediate and evident increase of the action of the heart. It was then applied to the brain with the same effect. Watery solutions of opium and tobacco were also applied to both, with precisely the same effect as in the rabbit. The increase of action from the opium and tobacco was much less than from the spirit of wine, and was soon followed by a great diminution of action. The increase of action was least, and the diminution greatest from tobacco. On washing off the opium and tobacco with a wet sponge, the heart immediately beat more strongly. The different parts of this experiment were frequently repeated with the same result. It is remarkable that we could affect the motion of the heart by stimuli applied to the brain and spinal marrow, after they had ceased to produce any effect on the muscles of voluntary motion through the medium of the nervous system.

Exp. 16. This experiment only differed from the last in the cervical part of the spinal marrow and lower part of the brain being removed, and the stimuli applied only to that part of the brain which lies between the eyes of the frog. Spirit of wine, opium, and tobacco, thus applied, affected the motion of the heart quite as much, and precisely in the same way, as when they were applied to the entire brain and spinal marrow. When opium and tobacco were applied to the lower part of the spinal marrow, the motion of the heart appeared to be hardly at all affected by them. It was evidently increased when spirit of wine was applied to the same part.

We found in the foregoing experiments, that considerable

pressure, either on the brain or spinal marrow, produced little or no effect on the action of the heart. Its action could be influenced by stimuli applied to the brain and spinal marrow long after the circulation had ceased.

The peristaltic motion of the intestines, as far as we could judge from the following experiments, obeys the same laws as the action of the heart.

Exp. 17. A rabbit was deprived of sensibility by a blow on the occiput. The whole of the spinal marrow was then destroyed by a hot wire. On opening the abdomen, we found the peristaltic motion of the stomach and small and great intestines quite as strong as when the nervous system is entire, as we ascertained by exposing the abdominal viscera of other rabbits. In another experiment, the spinal marrow was wholly removed, without at all affecting this motion. The removal of the brain, we found, produces as little effect upon it, as that of the spinal marrow. When both were removed at the same time, it remained unaffected. It continues till the intestines become cold, so that when the portions exposed to the air have lost their power, the motion of the parts beneath still remains.

We endeavoured to ascertain how far this motion is influenced by stimuli applied to the brain and spinal marrow, but from its nature it is in every way so irregular, that no certain result can be obtained. It often appeared to us, that spirit of wine applied to the brain and spinal marrow increased it.

The admission of air into the cavity of the abdomen throws the bowels into strong spasmodic action, which alone would obscure any effect that can be supposed to arise from stimulating the brain. To remove this cause of failure, the abdomen

was opened under tepid water; but this was found to excite even stronger spasms than the air had done.

What are the simple results of the foregoing experiments? The first set prove, that the power of the heart is independent of the brain and spinal marrow, for we find that it continues to perform its function after they are destroyed or removed, and that their removal is not attended with any immediate effect on its motions. The second set prove, that the action of the heart may be influenced by agents applied to any considerable portion either of the brain or spinal marrow. It is as readily influenced by agents applied to the anterior part of the brain, as by those applied to the cervical part of the spinal marrow. This is what we should expect when we trace the various origins of its nerves.

If it be said that the results of these experiments imply a contradiction, that we cannot suppose the power of the heart to be wholly independent of the brain and spinal marrow, and yet influenced by stimuli applied to them, the reply is, that such are the facts, of the truth of which any one may easily satisfy himself. Daily occurrences correspond with these facts. We rarely see the action of the heart destroyed by injuries of the brain and spinal marrow, unless they are such as interrupt respiration; yet its action is constantly influenced by affections of the mind.

On a closer examination of the phenomena of the nervous system, we shall find other similar difficulties. The experiments of M. LE GALLOIS prove, in the most satisfactory manner, that a principal function of the spinal marrow is to excite the muscles of voluntary motion, and that it can perform this office independently of the brain. It performs it

after the brain is wholly removed, and its powers seem not at all immediately impaired by the removal of the brain; yet we constantly see injuries of the brain impairing the functions of the spinal marrow. We may wholly remove the brain, and the animal performs the various motions of its limbs as well as before its removal. Yet an injury of the brain often produces complete hæmiplegia, nay often instantly destroys every function of the system. Of this apparent inconsistency, M. LE GALLOIS justly remarks, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove the imperfection of our knowledge.

Whichever of the disputed opinions respecting the functions of the nervous system we adopt, the foregoing phenomena seem to imply a contradiction; for an explanation of them, therefore, we must recur to principles different from those hitherto assumed. The following experiments point out still another instance of this apparent contradiction, and seem to suggest the principle on which the whole depends.

Exp. 18. By applying strong stimuli to the spinal marrow of a frog, strong and repeated contractions were excited in the muscles of the hind limbs, as long as the stimuli would produce the effect. On examining the state of the muscles of these limbs, I found them wholly deprived of their excitability. Now it is well known, that although all the nerves supplying the limbs of a frog be divided, and cut out close to the place where they enter the muscles, the latter still retain their excitability, which appears to be not at all less than while the nerves are entire. Lest it may be supposed that the nervous influence, which was exhausted in this experiment by stimulating the spinal marrow, still remains in

the muscles after the nerves are divided, and thus preserves their excitability, the following experiment was made.

Exp. 19. All the nerves supplying one of the hind limbs of a frog were divided, so that it became completely paralytic. The skin was removed from the muscles of the leg, and salt sprinkled upon them, which, being renewed from time to time, excited contractions in them for twelve minutes; at the end of this time they were found no farther capable of being excited. The corresponding muscles of the other limb, in which the nerves were entire, and of which consequently the animal had a perfect command, were then laid bare, and the salt applied to them in the same way. In ten minutes they ceased to produce any contractions, and the animal had lost the command of them. The nerves of this limb were now divided, as those of the other had been, but the excitability of the muscles to which the salt had been applied was gone. Its application excited no contraction in them. It sometimes happens, while the nerves of the limb are entire, that the voluntary efforts of the animal prevent the contractions usually excited by the application of salt. This experiment was repeated in the same manner, and with a similar result. After the experiment, the muscles of the thighs in both limbs were found to contract forcibly on the application of salt. It excited equally strong contractions on both sides.

It is remarkable, that in this experiment, the excitability of the muscles whose nerves were entire, was soonest exhausted. In the repetition of the experiment, this was the case to a still greater degree, the muscles, whose nerves were entire, losing their excitability in about one half of the time required for exhausting the other.

From this experiment it is evident, that the nervous influence, so far from having a power of preserving the excitability of the muscles, exhausts it like other stimuli. The excitability therefore is a property of the muscle itself. Yet we have just seen, that it may be wholly destroyed by changes induced on the nervous system. On the same principle we explain the seeming contradiction respecting the action of the heart. We have seen that its power exists as independently of the brain and spinal marrow, as the action of the first muscles to which the salt was applied, whose nerves had been divided; but, while the brain and spinal marrow retain their functions, and the connection of nerves is entire, the heart, as well as the muscles of voluntary motion, may be influenced by agents acting through the nervous system. It is not difficult to account for the latter being more copiously supplied with nerves than the heart, because all the stimuli which affect them, act through their nerves, while the heart is only now and then influenced through its nerves, its usual stimulus being as immediately applied to it, as the salt was to the muscles of the limb in the above experiment, and acting as independently of the nervous system. We do not surely in all this see any difference in the nature of the muscular power of the heart, and that of the muscles of voluntary motion, except their being fitted to obey different stimuli, a difference which we find in the two sides of the heart itself.

It may here be objected, that in apoplexy the power of the muscles of voluntary motion is lost, while that of the heart is little or not at all impaired. Were such the fact, this objection would be unanswerable; but I have repeatedly examined the state of the muscles of voluntary motion in apoplexy,

both in the warm and cold blooded animals, and found their excitability unimpaired. It is not their power, but the stimulus which excites them, that is lost in apoplexy. In this disease, the heart continues to contract, because its stimulus is still supplied; the muscles of voluntary motion cease to contract, because their stimulus is withdrawn.

By the foregoing experiments we arrive at the conclusion of HALLER, that the heart and other muscles possess an excitability independent of the nervous system; but we are carried a step farther, and taught that they are all equally capable of being stimulated through this system, by which the great objections to HALLER'S doctrine are removed. We may, I think, trace the subject still farther. It has been shown by direct experiment by M. LE GALLOIS, that the spinal marrow is capable of performing its functions independently of the brain, yet, as has just been observed, the spinal marrow may be influenced through the brain. Thus the excitability of the spinal marrow bears the same relation to the brain, which that of the muscles bears to the spinal marrow and its nerves, and I would add all nerves distributed to muscles, some of which arise from the brain, but seem to bear precisely the same relation to the sensorium with those which arise from the spinal marrow. Even M. LE GALLOIS, although his experiments lead to an opposite conclusion, observes, that the brain seems to act on the spinal marrow as the latter does on the parts it animates. We know the peculiar office of the brain, by observing what functions are lost by its removal, the sensorial functions. The nervous, then, obeys the sensorial system, in the same way in which the muscular obeys the nervous system, but as the muscular system has an existence independent of the

nervous, so has the nervous, independent of the sensorial system.

What is here said is finely illustrated by reviewing the various classes of animals. In the lowest class we find only the muscular system, which exists without either nervous system or sensorium. In the next class we find the muscular and nervous systems, which exist without sensorium. In the most perfect animals, we find the three vital powers combined, each having an existence not immediately depending on the others, but all so connected, that none can exist long without the others. The nature of this connection is obvious, when we consider that all are supported by the circulation, which depends for its immediate support on the muscular system, and cannot long exist without respiration, and that this depends not on the sensorium, but, as M. LE GALLOIS has satisfactorily proved, on the nervous system, which system is under the immediate influence of the sensorium, directing, but not producing, its various movements; and such is the power of the sensorium over the nervous system, that its affections may, through this system, at once destroy every function of life. Thus joy and other strong passions have killed more speedily than suffocation can, and therefore otherwise than through the destruction of respiration.

Exp. 20. All that has been said of the vital power of the heart is strikingly confirmed by the following experiments. If the head and spine of a frog be removed, the heart continues to perform its function perfectly for many hours, nor does it seem at all immediately affected by their removal. But we find the effect very different when the most sudden and powerful agent is applied to them. If they are destroyed by being cut

to pieces, or even by a hot wire, the heart after their destruction beats just as before it. But if either the brain or spinal marrow be instantly crushed, the heart immediately feels the shock.

The thorax of a large frog was laid open, and the motion of the heart observed, which performed the circulation perfectly, and with great force. The brain was then crushed by the blow of a hammer. The heart immediately performed a few quick and weak contractions. It then lay quite still for about half a minute. After this, its beating returned, but it supported the circulation very imperfectly. In ten minutes its vigour was so far restored that it again performed the circulation with freedom, but with less force than before the destruction of the brain. An instrument was then introduced under the heart, and after ascertaining that this had produced no change on its action, the spinal marrow was crushed by one blow, as the brain had been. The heart again beat quickly and feebly for a few seconds, and then seemed wholly to have lost its power. In about half a minute it again began to beat, and in a few minutes acquired considerable power, and again supported the circulation. It beat more feebly, however, than before the spinal marrow was destroyed. It ceased to beat in about an hour and a half after the brain had been destroyed. In another frog, after the brain and spinal marrow had been wholly removed, the heart beat nine hours, gradually becoming more languid.

In this experiment we see that the heart not only retains its power long after the brain and spinal marrow are removed, but that if they are destroyed in such a way as to impair and almost destroy the action of the heart, it can recover the power of performing its function, after they no longer exist; pre-

cisely as a muscle of voluntary motion will by rest recover its excitability, although all its nerves are divided, if its circulation continues.

M. Bichat (*Recherches Phys. sur la vie et la mort.*) has shown that in a frog the circulation continues in the capillaries after the heart no longer propels the blood.

Exp. 21. The foregoing experiment cannot be performed in the same way on warm-blooded animals, but it may be performed in a way equally satisfactory. In two rabbits the brain was crushed by a blow. In both the heart immediately beat with an extremely feeble and fluttering motion. The anterior part of the brain only was crushed in another rabbit, with the same result. A strong ligature was thrown round the neck of a fourth rabbit, and at the moment it was tightened, the head was cut off. The bleeding was restrained by the ligature, except from the vessels defended by the bone. General spasms made the body hard for the space of between one and two minutes, so that the beating of the heart could not be felt. At the end of this time, the heart was felt through the side, both by Mr. Hastings and myself, beating regularly, and not more quickly than in health. All the rabbits used in this experiment were of the same age.

Exp. 22. The following experiment is still more conclusive. The anterior part of the brain of a rabbit was crushed by a blow. The side was rendered hard by spasm for about half a minute. Neither during this, nor after it, could I perceive any motion of the heart by applying the hand to the side. The head was then cut off, about three quarters of a minute after the brain had been crushed. No blood spouted out, and very little ran from the vessels. A strong ligature

was passed round the neck of another rabbit of the same age. It was suddenly tightened, and the head cut off. In this instance little spasm took place, and the heart was found beating regularly under the finger for about three quarters of a minute. At the end of this time, the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force, till nearly the whole blood was evacuated.

Exp. 23. From the strength of the spine of a rabbit, and the situation of the neighbouring parts, it is impossible to crush it, without directly influencing the state of the heart by the blow. We opened it between the cervical and dorsal vertebræ, and suddenly forced a steel rod through the cervical part. As in the experiments of M. LE GALLOIS, the action of the heart was immediately debilitated. In the preceding experiments, the reader has seen, we repeatedly, slowly destroyed, or removed entirely, both the cervical and other portions of the spinal marrow, without at all influencing the action of the heart.

These experiments point out an easy solution of the difficulties mentioned by M. LE GALLOIS in the 119th and following pages of his Treatise. When the whole spinal marrow was destroyed by small portions at a time, comparatively little effect was produced on the heart; but when a considerable part of it was crushed at once, the power of the heart was so impaired, that circulation ceased. So in other cases, where the injury was inflicted slowly, and where it was inflicted suddenly, the result was found to be different.

Thus he observes, that if the spinal marrow be divided near the occiput, and a certain part of it immediately destroyed,

circulation ceases. If some time intervene between the division and the destruction of precisely the same part, the circulation is not interrupted.

M. le Gallois' explanation of these facts cannot surely be admitted, and indeed is inconsistent with his own positions. He found, that confining the circulation to a less extent, by throwing ligatures round the large vessels at some distance from the heart, enables this organ to support the circulation under circumstances where it would otherwise have failed. Writers on midwifery have, on the same principle, recommended compressing the arteries of the limbs when the powers of the heart are much weakened by hemorrhagy. From this experiment, compared with others, M. le Gallois infers, that when the spinal marrow is destroyed by small portions, the circulation, in the parts corresponding to these portions, being impeded, the effect is similar to that produced by the ligatures. Now, although it were ascertained that the circulation is impeded in any part by destroying the portion of the spinal marrow from which it is supplied with nerves, which I think may easily be shown not to be the case, this explanation would still be in opposition to M. le Gallois' fundamental position: "Que la quantité, que le contingent de forces, que chaque "portion de moëlle fournit à cet organe, égale pour le moins "celles dont il auroit strictement besoin pour entretenir la "circulation dans les seules parties correspondantes à cette "portion." When the ligatures were thrown around the vessels, the heart was deprived of none of its supposed nervous influence. When, on the contrary, portions of the spinal marrow were successively destroyed, as far as this is supposed to confine the circulation, it must also, according to M. le

GALLOIS, occasion a loss of power in the heart. He remarks that till the above explanation occurred to him, he had resolved to abandon this part of the inquiry. "Après bien "des efforts inutiles pour porter la lumière dans cette téné- "breuse question, je pris le parti de l'abandonner, non sans "regret d'y avoir sacrifié un grand nombre d'animaux, et "perdu beaucoup de temps." Just before, he observes, "En "un mot, j'eus presque autant de résultats différens que d'ex- "périences." This may be easily accounted for, as he was not aware that the rapidity with which any portion of the spinal marrow is destroyed, influences the result. We also see why the sudden destruction of one half of the spinal marrow, after it had been divided, not only brought death to that part of the animal to which it belonged, but to the other also; a fact which seems in direct opposition to M. LE GALLOIS' explanation of that we have just been considering.

In M. LE GALLOIS' experiments, the spinal marrow was always crushed by a stilet, of precisely the same dimensions with the cavity of the spine. In the foregoing experiments, the spinal marrow was either removed or destroyed by a comparatively small wire moved about in it till all its functions ceased. The reader will easily understand, from what has been said, why this apparently slight circumstance occasions so essential a difference in the result of the experiments. We have just seen the difference of the result when any portion of the spinal marrow is successively destroyed by parts, or crushed at once, and when the brain is crushed at once or wholly removed.

We have every reason to believe, from the experiments which have been related, that the peristaltic motion of the

bowels obeys the same laws as the action of the heart. It appears from those experiments, that this motion is wholly independent of the nervous system. It continues till the parts become cold after the brain and spinal marrow are removed. I have already mentioned the circumstances which prevented our positively ascertaining, whether it is influenced by stimuli applied to the brain and spinal marrow, (Exp. 17.) but we know that the action of the bowels is frequently influenced by affections of the mind.

From the whole of the foregoing experiments and observations, it appears,

1. That the muscles of involuntary motion obey the same laws with those of voluntary motion. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 12, 13, 14, 15, 16, and 17, compared with Exp. 18, and 19; see also observations under Exp. 19.

2. That the apparent difference in the nature of these muscles, arises from their being under the influence of different stimuli; see observations under Exp. 19.

3. That they are both capable of being stimulated through the nervous system. Exp. 12, 13, 14, 15, 16.

4. That the power of both is independent of the nervous system. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 17, 18, and 19.

5. That what is called the nervous system consists of two parts, whose existence is not immediately dependent on each other; the one performing the sensorial functions, the other conveying impressions to and from the sensorium, and, without bestowing any power on the muscular system, acting as a stimulus to it. See the observations under Exp. 19. See also Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 18, 19.

6. That there is therefore in the most perfect animals a combination of three distinct vital powers, not immediately depending on each other ; one of the muscular system, one of the nervous system properly so called, and one of the sensorial system. See observations under Exp. 19.

7. That the muscular system, though independent of the nervous system, is so influenced by it, that the power of the former may even be destroyed through the nervous system. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, compared with Exp. 18, 20, and 23.

8. That both the muscular and nervous systems, though independent of the sensorial system, are so influenced by it, that they may even be destroyed through it. Exp. 8, 10, 11, 17, and two last paragraphs under Exp. 19, compared with Exp. 20, 21, 22.

9. That although in the less perfect animals we find the muscular life existing alone, and the muscular and nervous existing without the sensorial life; in the more perfect animals they are so connected, that none can exist long without the others. See the last paragraph under Exp. 19.

10. That nutrition, circulation, and respiration, are the means by which they are so connected.

Worcester, August 16th, 1814.

VII. *Experiments to ascertain the influence of the spinal marrow on the action of the heart in fishes. By Mr.* William Clift. *Communicated by Sir* Everard Home, *Bart. V. P. R. S.*

Read February 16, 1815.

As the experiments of M. Gallois, which led him to conclude, that the action of the heart is dependent upon the spinal marrow, were principally made upon quadrupeds, in which death is so readily produced, when the vital organs are injured, or any one of them destroyed; I thought by repeating those experiments upon fishes, which are much more tenacious of life, and whose mode of respiration is of a more simple nature, the truth or fallacy of his conclusions might be ascertained.

I was led to select the carp for this purpose, having observed that a carp after the head was cut off, and the heart had been removed for above four hours, and the fish was considered to have been long dead, upon being put into hot water, leaped out of the vessel with a degree of vigour totally unexpected, being equal to the struggles of a living fish.

I shall not take up the time of the Society, by a detail of all the experiments I made, but shall only select those which bear upon this particular subject.

Exp. 1. Two carp of nearly equal size, had their hearts exposed, by opening into the pericardium. In one of them, the heart on exposure pulsated twenty times in a minute, but soon after, only twelve. The fish was put into river water,

in which it swam, with the tail, fins, and gills in full action; in forty minutes the fish turned upon its side, the action of the heart weak, the frequency of pulsations the same; the fins ceased to act, the tail continued to act feebly, the gills strongly.

In an hour, the action of the heart was weaker, the frequency of pulsations the same.

In two hours, the action of the heart and gills was not perceptible in the water, but when taken out and examined, the gills had a feeble action by irregular jerks; the pulsations of the heart were very weak, and rather more frequent.

In three hours, the heart and gills had ceased to act, and the muscles of the body did not contract when stimulus was applied to them.

In the other carp, the heart pulsated eight times in a minute. The fish was laid upon a moist leaden tray. In one hour the pulsations were ten in a minute; in an hour and half, twelve, the fins and tail in full motion. In two hours, the pulsations were a little weaker, but of the same frequency. In three hours and forty minutes the action of the heart ceased. The gill covers acted fifteen minutes longer.

Exp. 2. Having exposed the heart of a carp as in the former experiment, the pulsations were found to be eight in a minute. In eleven minutes, a red hot wire was passed from the tail to the occiput, so as to destroy the spinal marrow; the action of the heart was immediately quickened for three beats; then a long pause, after which the pulsations were the same as before. All action ceased in the muscles behind the pectoral fins, and those muscles did not contract, when stimulus was applied to them.

In one hour from the laying open the pericardium, and five minutes from the passing of the wire, the pulsations had the same frequency.

In twenty minutes after passing the wire, the pulsations were strong, and fifteen in a minute.

In forty-five minutes, the pulsations were twelve in a minute, exactly corresponding with those of the heart of the carp laid on the wet tray, in which the spinal marrow remained entire.

The brain was now exposed, the pulsations became fifteen in a minute, but in five minutes were only twelve. The brain was then extracted, the pulsations of the heart were not sensibly affected either in strength or frequency. The gills and mouth had no longer the least degree of action. In two hours from the beginning of the experiment, the pulsations were fifteen in a minute.

Turning the fish always accelerated the action of the heart a little.

In three hours, the pulsations were the same.

In four hours, they were twelve in a minute.

In five hours, they varied from twelve to six in a minute.

In six hours, they were six in a minute.

In seven hours, nine in a minute.

From this time the action of the heart was very slow and weak: In eight hours and a half, once in a minute.

In eleven hours and forty minutes, they ceased entirely.

Exp. 3. A carp had the heart exposed, by laying open the pericardium: The pulsations were twenty in a minute, but soon were only twelve.

In forty minutes, the spinal marrow at the occiput was cut

through, which increased the action of the heart for three or four beats, which were very violent, and at the rate of twenty in a minute; the pulsations were then twelve as before.

In fifty minutes, a red hot iron wire was passed from the tail to the occiput, and produced strong action in the tail; but from this time, there was no action in the muscles of the body or tail. The pulsations of the heart became a little slower. In sixty minutes they were strong, and twelve in a minute.

The brain was now broke down by a small flat pointed instrument; the pulsations of the heart became twenty in a minute. The gills and mouth from this time ceased to act. The heart went on pulsating with the same frequency as before, but weaker, for two hours, and then gradually ceased; the auricle contracting for more than a quarter of an hour after the action of the ventricle had ceased.

Exp. 4. A weakly carp of nearly the same length with those employed for the other experiments, which was fifteen inches, had the spinal marrow exposed near the base of the tail, and a red-hot iron wire passed along the vertebral canal. All action of the body and gills immediately ceased.

On removing a portion of the skull to expose the brain, the end of the wire was found in the skull, but the brain uninjured. The brain was then entirely removed: no motion was produced in any part of the body or gills.

The body was placed in a wet towel for three hours. The heart was then exposed, its action was strong and distinct in both the auricle and ventricle, at the rate of twenty beats in a minute, but after five pulsations, at the rate of twelve, at which it continued.

At three hours and a half, the action of the heart was weaker

and slower, there being an interval of one second between the contraction of the auricle and ventricle. From the time of exposing the heart no action was perceptible in the branchial artery; it remained of a blue colour, and turgid with blood from its first exposure.

At three hours and three quarters, the pulsations of the heart were only nine in a minute. The interval between the contraction of the auricle and ventricle three seconds.

At four hours, the pulsations were seven in a minute, the interval between the contraction of the auricle and ventricle four seconds.

At four hours and a quarter, the pulsations were seven in a minute; the interval between the contraction of the auricle and ventricle five seconds.

At four hours and a half, seven in a minute; the interval between the contraction of the auricle and ventricle six seconds.

At four hours and three quarters, seven in a minute; the auricle and ventricle acting together.

At five hours, seven in a minute, the interval between the contraction of the auricle and ventricle, one second and a half.

At six hours, the contraction of the auricle seven in a minute, the action of the ventricle hardly perceptible.

At six hours and a quarter, the contractions of the auricle weak: seven in a minute.

At six hours and a half, the auricle ceased to contract; and after this time, no stimulus produced the smallest action in any part of the heart. This experiment was suggested by a notice taken from Dr. Wilson Philip's paper, published in

the Edinburgh Medical and Chirurgical Journal for January last, and made many months after the other experiments.

From these experiments we have the following results:

1. That the muscles of the body of a carp four hours after the brain and heart are removed, can be thrown into powerful action.

2. That the moment the spinal marrow is destroyed, these muscles lose all power of action.

3. That when water is admitted into the pericardium, and the fish allowed to swim about, the action of the heart ceases sooner than when that organ is exposed to air, and the fish kept quiet.

4. That whether the heart is exposed or not, its action continues long after the spinal marrow and brain are destroyed, and still longer when the brain is removed without injury to its substance.

5. That the action of the heart is accelerated for a few beats, by exposure of that organ; by exposure of the brain; injury to the brain; destruction of the spinal marrow while connected with the brain; by the connection between the brain and spinal marrow being cut off: while removing the whole brain produces no sensible effect upon the heart's action, and destroying the spinal marrow after it is separated from the brain renders the action of the heart slower for a few beats.

VIII. *Some experiments and observations on the colours used in painting by the Ancients. By Sir* Humphry Davy, *LL. D. F.R.S.*

Read February 23, 1815.

I. *Introduction.*

THE importance the Greeks attached to pictures, the estimation in which their great painters were held, the high prices paid for their most celebrated productions, and the emulation existing between different states with regard to the possession of them, prove that painting was one of the arts most cultivated in ancient Greece; the mutilated remains of the Greek statues, notwithstanding the efforts of modern artists during three centuries of civilization, are still contemplated as the models of perfection in sculpture, and we have no reason for supposing an inferior degree of excellence in the sister art, amongst a people to whom genius and taste were a kind of birthright, and who possessed a perception, which seemed almost instinctive, of the dignified, the beautiful, and the sublime.

The works of the great masters of Greece are unfortunately entirely lost. They disappeared from their native country during the wars waged by the Romans with the successors of Alexander, and the later Greek republics; and were destroyed either by accident, by time, or by barbarian conquerors at the period of the decline and fall of the Roman Empire.

The subjects of many of these pictures are described in

ancient authors, and some idea of the manner and style of the Greek artists may be gained from the designs on the vases, improperly called Etruscan, which were executed by artists of Magna Græcia, and many of which are probably copies from celebrated works: and some faint notion of their execution and colouring may be gained from the paintings in fresco found at Rome, Herculaneum, and Pompeii.

These paintings, it is true, are not properly Greek, yet whatever may be said of the early existence of painting in Italy as a native art, we are certain that at the period when Rome was the metropolis of the world, the fine arts were cultivated in that city exclusively by Greek artists, or by artists of the Greek schools. By comparing the descriptions of VITRUVIUS* and PLINY with those of THEOPHRASTUS,† we learn that the same materials for colouring were employed at Rome and at Athens; and of thirty great painters that Pliny mentions whose works were known to the Romans, two only are expressly mentioned as born in Italy, and the rest were Greeks. Ornamental fresco painting was indeed generally exercised by inferior artists; and the designs on the walls of the houses of Herculaneum and Pompeii, towns of the third or fourth order, can hardly be supposed to offer fair specimens of excellence, even in this department of the art: but in Rome, in the time of her full glory, and in the ornaments of the imperial palace of the first Cæsars, all the resources of the distinguished artists of that age were probably employed. PLINY names CORNELIUS PINUS and ACCIUS PRISCUS as the two artists of the greatest merit in his own time, and states that they painted the Temple of Honour and Virtue,‡ " Imperatori

* De Architectura, Lib. vii. Cap. 5.

† De Lapidibus.

‡ Plin. Nat. Hist. Lib. xxxv. Cap. 37.

Vespasiano Augusto restituenti," and it is not improbable, that these artists had a share in executing, or directing the execution of the paintings and ornaments in the baths of Titus; and at this period the works of Zeuxis, Parrhasius, Timanthes, Apelles, and Protagoras were exhibited in Rome, and must have guided the taste of the artists. The decorations of the baths were intended to be seen by torch light, and many of them at a considerable elevation, so that the colours were brilliant and the contrast strong; yet still these works are regarded by connoisseurs as performances of considerable excellence: the minor ornaments of them have led to the foundation of a style in painting which might with much more propriety be called Romanesque than Arabesque: and no greater eulogy can be bestowed upon them than the use to which they have been applied by the greatest painter of modern times, in his exquisite performances in the Vatican. In these and in other works of the same age, the effect of the ancient models is obvious; and the various copies and imitations that have been made of these remains of antiquity have transferred their spirit into modern art, and left little to be desired as to those results which the skill of the painter can command. There remains, however, another use to which they may be applied, that of making us acquainted with the *nature* and *chemical composition* of the colours used by the Greek and Roman artists. The works of Theophrastus, Dioscorides, Vitruvius, and Pliny, contain descriptions of the substances used by the ancients as pigments; but hitherto, I believe, no experimental attempt has been made to identify them, or to imitate such of them as are peculiar.* In the

* In the 70th Volume of the Annales de Chimie, page 22, M. Chaptal has

following pages I shall have the honor of offering to the Society an investigation of this subject. My experiments have been made upon colours found in the baths of Titus, and the ruins called the baths of Livia, and in the remains of other palaces and baths of ancient Rome, and in the ruins of Pompeii. By the kindness of my friend, the celebrated Canova, who is charged with the care of the works connected with ancient art in Rome, I have been enabled to select, with my own hands, specimens of the different pigments that have been found in vases discovered in the excavations, lately made beneath the ruins of the palace of Titus, and to compare them with the colours fixed on the walls or detached in fragments of stucco: and Signor Nelli, the proprietor of the Nozze Aldobrandine, with great liberality permitted me to make such experiments upon the colours of this celebrated picture, as were necessary to determine their nature. When the preservation of a work of art was concerned, I made my researches upon mere atoms of the colour, taken from a place where the loss was imperceptible: and without having injured any of the precious remains of antiquity, I flatter myself, I shall be able to give some information not without interest to scientific men as well as to artists, and not wholly devoid of practical applications.

published a paper on seven colours found in a colour shop at Pompeii. Four of these he found to be natural colours, ochres, a specimen of Verona green, and one of pumice stone. Two of them were blues, which he considers as compounds of alumine and lime with oxide of copper, and the last a pale rose colour, which he regards as analogous to the lake formed by fixing the colouring matter of madder upon alumine. I shall again refer to the observations of M. Chaptal in the course of this paper. It will be found on perusal, that they do not supersede the enquiry mentioned in the text.

II. *Of the red colours of the Ancients.*

Amongst the substances found in a large earthen vase filled with mixtures of different colours with clay and chalk, found about two years ago in a chamber at that time opened in the baths of Titus, are three different kinds of red. One bright and approaching to orange, another dull red, a third a purplish red.* On exposing the bright red to the flame of alcohol, it became darker red, and on increasing the heat by a blowpipe, it fused into a mass having the appearance of litharge, and which was proved to be this substance by the action of sulphuric and muriatic acids. This colour is consequently minium, or the red oxide of lead.

On exposing the dull red to heat, it became black, but on cooling recovered its former tint. When heated in a glass tube it afforded no volatile matter condensible by cold but water. Acted on by muriatic acid, it rendered it yellow, and the acid, after being heated upon it, yielded an orange coloured precipitate to ammonia. When fused with hydrate of potassa, the colour rendered it yellow; and the mixture acted on by nitric acid afforded silica and orange oxide of iron. It is evident from these results that the dull red colour is an iron ochre.

The purplish red submitted to experiments, exhibited similar phenomena, and proved to be an ochre of a different tint.

In examining the fresco paintings in the baths of Titus, I found that these colours had been all of them used, the ochres particularly, in the shades of the figures, and the minium in the ornaments on the borders.

I found another red on the walls, of a tint different from

* Nearly of the same tint as prussiate of copper.

those in the vase and much brighter, and which had been employed in various apartments, and formed the basis of the colouring of the niche and other parts of the chamber in which the Laocoon is said to have been found. On scraping a little of this colour from the wall, and submitting it to chemical tests, it proved to be vermilion or cinnabar, and on heating it with iron filings, running quicksilver was procured from it.

I found the same colour on some fragments of ancient stucco in a vineyard near the pyramidal monument of CAIUS CESTIUS.

In the Nozze Aldobrandine, the reds are all ochres. I tried on these reds the action of acids, of alkalies, and of chlorine, but could discover no traces either of minium or vermilion in this picture.

Minium was known to the Greeks under the name of σανδαράχη,* and to the Romans under that of cerussa usta. It is said, by PLINY,† to have been discovered accidentally by means of a fire that took place at the Piræeus at Athens. Some ceruse which had been exposed to this fire was found converted into minium, and the process was artificially imitated: and he states that it was first used as a pigment by NICIAS.‡

Several red earths used in painting are described by THEOPHRASTUS, VITRUVIUS,§ and PLINY. The Sinopian earth, the Armenian earth, and the African ochre, which had its red colour produced by calcination.

Cinnabar or vermilion was called by the Greeks κιννάβαρι,‖ and by the Romans minium. It is said by THEOPHRASTUS¶ to have been discovered by CALLIAS, an Athenian, ninety years before PRAXIBULUS, and in the 349th year of Rome, and was

* Dioscorides, Lib. v. 122.

† Lib. xxxv. Cap. 20.

‡ Pliny, Lib. xxxv. Cap. 20.

§ De Architectura, Lib. vii. Cap. 7.

‖ Dioscorides, Lib. v. Cap. 109.

¶ De Lapid. Cap. 104.

prepared by washing the ores of quicksilver. According to PLINY,* who quotes VERRIUS, it was a colour held in great esteem in Rome at the time of the Republic; on great festivals it was used for painting the face of Jupiter Capitolinus, and likewise for colouring the body of the Victor in the triumphal processions, " sic Camillum triumphasse."† PLINY mentions that even in his time vermilion was always placed at triumphal feasts amongst the precious ointments; and that the first occupation of new censors of the Capitol was to fill the place of vermilion painter to Jupiter.

Vermilion was always a very dear colour amongst the Romans; and we are informed by PLINY that to prevent the price from being excessive, it was fixed by the government. The circumstance of the chambers in the baths of Titus being covered with it, affords proof in favour of their being intended for imperial use; and we are expressly informed by the author I have just quoted, that the Laocoon, in his time, was in the palace of Titus:‡ and the taste of the ancients in selecting a colour to give full effect to their master pieces of sculpture was similar to that of a late celebrated English connoisseur.

PLINY describes a second or inferior sort of vermilion formed by calcining stone found in veins of lead. It is evident, that this substance was the same as our minium, and the Roman cerussa usta, and the stones alluded to by PLINY must have been carbonate of lead: and he states distinctly, that it is a substance which becomes red only when burnt.

* Lib. xxxiii. Cap. 36. Nunc inter pigmenta magnæ auctoritatis, et quondam apud Romanos non solum maximæ, sed etiam sacræ. † Ibid.

‡ Lib. xxxvi. Cap. 4. Sicut in Laocoonte, qui est in Titi Imperatoris domo, opus omnibus et picturæ et statuariæ artis præponendum.

III. *Of the yellows of the ancients.*

A large earthen pot found in one of the chambers of the baths of Titus contains a quantity of a *yellow paint*, which, submitted to chemical examination, proved to be a mixture of yellow ochre with chalk or carbonate of lime.

This colour is used in considerable quantities in different parts of the baths; but principally in the least ornamented chambers, and in those which were probably intended for the use of the domestics. In the vase to which I alluded in the last section, I found three different yellows; two of them proved to be yellow ochres mixed with different quantities of chalk, and the third a yellow ochre mixed with red oxide of lead, or minium.

The ancients procured their yellow ochre* from different parts of the world, but the most esteemed, as we are informed by Pliny, was the Athenian ochre; and it is stated by Vitruvius, that in his time the mine which produced this substance was no longer worked.

The ancients had two other colours which were orange or yellow; the auripigmentum, or *ἀρσενικον*, said to approach to gold in its colour, and which is described by Vitruvius† as found native in Pontus, and which is evidently sulphuret of arsenic; and a *pale* sandarach, said by Pliny to have been found in gold and silver mines, and which was imitated at Rome by a partial calcination of ceruse, and which must have been massicot, or the yellow oxide of lead mixed with minium. That there was a colour called by the Romans sandarach, different from pure minium, is evident from what

* *ὠχρα*, Theophrastus de Lapidibus.

† Vitruvius, Lib. vii.

Pliny says, namely, that the palest kind of orpiment resembles sandarach, and from the line of Naevius, one of the most ancient Latin poets, " Merula sandaracino ore:" so that this colour must have been a bright yellow similar to that of the beak of the blackbird.* Dioscorides describes the best σανδαράχη as approaching in colour to vermilion,† and the Greeks probably always applied this term to minium; but the Romans seem to have used it in a different sense; and some confusion was natural when different colours were prepared from the same substance by different degrees of calcination.

I have not detected the use of orpiment in any of the ancient fresco paintings; but a deep yellow approaching to orange, which covered a piece of stucco in the ruins near the monument of Caius Cestius, proved to be oxide of lead, and consisted of massicot mixed with minium. It is probable, that the ancients used many colours from lead of different tints between the usta of Pliny, which was our minium, and imperfectly decomposed ceruse, or pale massicot.

The yellows in the Aldobrandini picture are all ochres. I examined the colours in a very spirited picture, on the wall of one of the houses at Pompeii, of a lion and a man, they all proved to be red and yellow ochres.

IV. *Of the blue colours of the Ancients.*

Different shades of blue are used in the different apartments of the baths of Titus, and several very fine blues exist in the mixtures of colours to which I have referred in the last two sections.

These blues are pale or darker, according as they contain

* Histoire de la Peinture ancienne, page 199.

† Lib. v. 122.

larger or smaller quantities of carbonate of lime, but when this carbonate of lime is dissolved by acids, they present the same body colour, a very fine blue powder similar to the best smalt or to ultramarine, rough to the touch, and which does not lose its colour by being heated to redness; but which becomes agglutinated and semifused at a white heat.

This blue I found was very little acted on by acids. Nitro-muriatic acid by being long boiled upon it gained, however, a slight tint of yellow, and afforded proofs of the presence of oxide of copper.

A quantity of the colour was fused for half an hour with twice its weight of hydrate of potassa; the mass which was bluish green was treated by muriatic acid in the manner usually employed for the analysis of siliceous stones, when it afforded a quantity of silica equal to more than $\frac{3}{5}$ of its weight. The colouring matter readily dissolved in solution of ammonia, to which it gave a bright blue tint, and it proved to be oxide of copper. The residuum afforded a considerable quantity of alumine, and a small quantity of lime.

Amongst some rubbish that had been collected in one of the chambers of the baths of Titus, I found several large lumps of a deep blue frit, which when powdered and mixed with chalk produced colours exactly the same as those used in the baths, and which when submitted to chemical tests were found to be the same in composition.

The minute quantity of lime found in this substance was not sufficient to account for its fusibility: it was therefore reasonable to expect the presence of a fixed alkali in it; and on fusing some of it with three times its weight of boracic acid, and treating the mass with nitric acid and carbonate of am-

monia, and afterwards distilling sulphuric acid from it, I procured from it sulphate of soda, which proves that it was a frit made by means of soda, and coloured with oxide of copper.

The undiluted colour in its form of frit is used for ornamenting some of the mouldings detached from the ceilings of the chambers in the baths of Titus: and the walls of one chamber between the compartments of red marble, bear proofs of having been covered with this frit, and retain a considerable quantity of it.

There is every reason to believe, that this is the colour described by THEOPHRASTUS as discovered by an Egyptian king;* and of which the manufactory is said to have been anciently established at Alexandria.

VITRUVIUS speaks of it, under the name of cæruleum,† as the colour used commonly in painting chambers, and states, that it was made in his time at Puzzuoli, where the method of fabricating it was brought from Egypt by VESTORIUS; he gives the method of preparing it by heating strongly together sand, flos nitri,‡ and filings of copper.

PLINY mentions other blues, which he calls species of sand (arenæ) from the mines of Egypt, Scythia, and Cyprus. These natural blues, there is reason to believe, were different preparations of lapis lazuli, and of the blue carbonates and arseniates of copper.

Both PLINY and VITRUVIUS speak of the Indian blue, which the first author states to be combustible, and which was evidently a species of indigo.

I have examined several blues in the fragments of fresco

* De Lapidibus, sect. xcviii. † Lib. vii. Cap. 11.

‡ This identifies the nitrum of the ancients with carbonate of soda.

painting from the ruins near the monument of CAIUS CESTIUS. In a deep blue approaching in tint to indigo, I found a little carbonate of copper, but the basis of this colour was the frit before described.

The blues in the Nozze Aldobrandine, from their resisting the action of acids, and from the effects of fire, I am inclined to consider as composed of the Alexandrian or Puzzuoli blue.

In an excavation made at Pompeii, in May 1814, at which I was present, a small pot containing a pale blue colour was dug up, which the exalted personage, by whose command the excavation was made, was so good as to put into my hands. It proved to be a mixture of carbonate of lime with the Alexandrian frit.*

VITRUVIUS states, that the ancients had a mode of imitating the Indian blue or indigo, by mixing the powder of the glass called by the Greeks ὕαλος, with selinusian "creta" or annularian "creta", which was white clay or chalk mixed with stained glass; the same practice is likewise referred to by PLINY.

There is much reason for supposing that this stained glass, or ὕαλος, was tinged with oxide of cobalt; and that these colours were similar to our smalt. I have not found any powdered colour of this kind in the baths of Titus, or in any other Roman ruins; but a blue glass tinged with cobalt is very common in those ruins, which when powdered forms a pale smalt.

I have examined many pastes and glasses that contain oxide of copper; they are all bluish green, green, or of an opaque watery blue. The transparent blue glass vessels which are

* This probably is the same colour as that examined by M. CHAPTAL. He did not search in it for alkali, or there is every reason to suppose he would have found soda.

found with the vases in the tombs in Magna Græcia are tinged with cobalt, and on analyzing different ancient transparent blue glasses which Mr. MILLINGEN was so good as to give me, I found cobalt in all of them.*

THEOPHRASTUS, in speaking of the manufacture of glass, states as a report that "χαλκὸς" was used to give it a fine colour, and it is extremely probable, that the Greeks took cobalt for a species of χαλκὸς. I have examined some Egyptian pastes which are all tinged blue and green with copper, but though I have made experiments on nine different specimens of ancient Greek and Roman *transparent* blue glass, I have not found copper in any, but cobalt in all of them.†

V. *Of the ancient greens.*

The ceiling of the chambers called the Baths of Livia is highly ornamented with gilding and paintings; the larger paintings have been removed, but the ground work and the borders remain. A fragment detached from the borders, which appears of the same colour as the ground work, was of a deep sea green. The colouring matter examined, proved to be soluble in acids with effervescence, and when precipitated from acids, it redissolved in solution of ammonia, giving it the

* The mere fusion of these glasses with alkali and digestion of the product with muriatic acid was sufficient to produce a sympathetic ink from them; even the silica separated by the acid gained a faint blue green tint by heat, and the solution in muriatic acid became parmanently green by the action of sulphuric acid, a phenomenon Dr. MARCET has observed as belonging to the muriate of cobalt.

† A gentleman at Milan informed me last summer that he had found oxide of cobalt in the blue glass found in the ruins of Hadrian's villa, and at this time I had no idea that cobalt was known to the ancients. Mr. HATCHETT, and Mr. KLAPROTH had both found oxide of copper in some ancient blue glasses, which I conceive must have been opaque.

bright blue tint produced by oxide of copper. There are several different shades of green employed in the baths of Titus, and on the fragments found near the monument of Caius Cestius: in the vase of mixed colours I found three different varieties; one, which approached to olive, was the common green earth of Verona; another, which was pale grass green, had the character of carbonate of copper mixed with chalk; and a third, which was sea green, was a green combination of copper mixed with the blue copper frit.

All the greens that I examined on the walls of the baths of Titus were combinations of copper. From the extreme brilliancy of a green which I found in the vineyard to which I have so often referred, I suspected that it might contain arsenious acid, and be analogous to Scheele's green; but on submitting it to experiments, it afforded no indications of this substance, and proved to be a pure carbonate of copper.

The greens of copper were well known to the Greeks; the most esteemed is described by Theophrastus and Dioscorides under the name of *χρυσοκόλλα*, and is stated by both to be found in metallic veins.

Vitruvius mentions chrysocolla as a native substance found in copper mines, and Pliny speaks of an artificial chrysocolla made from the clay found in the neighbourhood of metallic veins, which clay was most probably impregnated with copper. He describes it as rendered green by the herb luteum. There is every reason to believe, that the native chrysocolla was carbonate of copper, and that the artificial was clay impregnated with sulphate of copper rendered green by a yellow die.

Some commentators have supposed that chrysocolla is the

same substance as borax, because PLINY has mentioned that a preparation called by this name was used by goldsmiths for soldering gold;* but nothing can be more gross than this mistake, which, however, has been copied into many elementary books of chemistry. The material used for soldering gold consisted of carbonate or oxide of copper mixed with alkaline phosphates. This is evident from the description of DIOSCORIDES "Περὶ ιοῦ σκώληκος Lib. v. c. 92, who says it was prepared from urine treated in brass mortars. PLINY says likewise, that it was prepared from "Cypria ærugine et pueri impubis urina, addito nitro."† The name of chrysocolla was probably derived from the green powder used by the goldsmiths, and which contained carbonate of copper as one of its ingredients.‡

Amongst the substances found in the baths of Titus were some masses of a grass green colour. I at first thought these might be specimens of native chrysocolla; they proved indeed to be carbonate of copper, but it had formed round longitudinal nuclei of red oxide of copper, so that probably these substances had been copper nails or small pieces of copper used in the building, converted by the action of the air, during so many centuries, into oxide and carbonate.

The ancients, as it appears from THEOPHRASTUS, were well

* Hist. de la Peinture ancienne, pag. 38. "Nos droguistes la nomme Borax."

† Lib. xxxiii. Cap. 5.

‡ The commentators have been likewise misled by PLINY's description, "chrysocolla humor est in puteis per venam auri defluens, &c." Ibid; but this is merely an inaccurate account of the decomposition of a vein containing copper. We have no reason for supposing that the Greeks and Romans were acquainted with borax. PLINY, probably misled by the application of the same name to different substances, considered chrysocolla as the cement of gold in mineral veins.

acquainted with verdigrise. VITRUVIUS mentions it amongst pigments, and probably many of the ancient greens, which are now carbonate of copper, were originally laid on in the state of acetite.

The ancients had beautiful deep green glasses, which I find are tinged with oxide of copper; but it does not appear that they used these glasses in a state of powder as pigments.

The greens of the Aldobrandini picture are all of copper, as was evident from the action of the muriatic acid upon them.

VI. *Of the purple of the Ancients.*

The Πορφύρα of the Greeks, and the ostrum of the Romans, was regarded as their most beautiful colour, and was prepared from shell fish.

VITRUVIUS* says, that the colour differed according to the country from which the shell fish was brought; that it afforded a colour deeper and more approaching to violet from the northern countries, and a redder colour from the southern coasts. He states, that it was prepared by beating the fish with instruments of iron, freeing the purple liquor from the shell containing it, and mixing it with a little honey: and PLINY says, that for the use of the painters argentine "creta"† was dyed with it: and both VITRUVIUS and PLINY say, that it was adulterated, or imitations of it made, by tinging "creta" with madder,‡ and "hysginum." The finest purple, PLINY

* Lib. vii. Cap. 13.

† Probably a clay used for polishing silver. The ancients were not acquainted with the distinction between aluminous and calcareous earths, and *creta* was a term applied to every white fine earthy powder.

‡ Madder was extensively used by the ancients in dying, and from this passage it is probable, that they were acquainted with the art of making a lake from it similar

says, had a tint like that of a deep coloured rose: and in painting, he states that it was laid on to give the last lustre to the sandyx, a composition made by calcining together red ochre and sandarach, and which therefore must have been nearly the same as our crimson.

In the baths of Titus there is a broken vase of earthen ware, which contains a pale rose colour; where it has been exposed to air, it has lost its tint, and is become of a cream colour, but the interior has a lustre approaching to that of carmine.

I have made many experiments on this colour. It is destroyed and becomes of a red brown by the action of concentrated acids and alkalies; but diluted acids dissolve a considerable quantity of carbonate of lime with which the body colour is mixed, and leave a substance of a bright rose colour: this substance when heated first blackens, and when urged with a strong flame becomes white; and treated with alkali, proves to be composed of siliceous, aluminous, and calcareous earths, with no sensible quantity of any metallic substance, except oxide of iron.

I endeavoured to discover if the colouring matter was combustible. It was gradually heated in a glass tube filled with oxygene; it did not inflame but became red hot sooner than it would have done had it been merely earthy matter: on exposing the gas in the tube to lime water, there was a precipitation of carbonate of lime. Some of it was mixed with hyperoxymuriate of potassa, and heated in a small retort;

to that used by modern painters. It was probably one of the colours used by the Egyptians in dying their stuffs of different colours from the same liquor, by means of mordants. If we can trust Pliny's account, they practised calico printing in a manner similar to the moderns. Lib. xxxv. Cap. 42.

when the salt fused there was a slight scintillation, a little moisture appeared, and the gas given off received into lime water occasioned a very evident precipitation.

It appeared from these experiments, that the colouring matter was a compound of either vegetable or animal origin. I threw some of it upon a hot iron, it emitted scarcely any smoke, and gave a smell which had some resemblance to that of prussic acid, but which was extremely faint.

When hydrate of potassa was fused in contact with it, the vapours that rose had no distinct ammoniacal smell; they gave indeed slight fumes to paper moistened with muriatic acid, but this is far from being an unequivocal proof of animal matter. I compared this colour with vegetable lake from madder, and animal lake from cochineal diluted to the same degree as nearly as could be judged, and fixed upon clays. The lake of madder, after being dissolved in strong muriatic acid, had its colour restored by alkalies, which was not the case with the ancient lake. The lake of madder likewise gave a much deeper tint to muriatic acid, and produced a tawny hue when its weak muriatic solution was acted on by muriate of iron; whereas the ancient lake did not change in colour. The ancient lake agreed with the lake of cochineal in being rendered of a deeper hue by weak alkalies, and of a brighter hue by weak acids; but it differed from it in being much more easily destroyed by strong acids. It agreed with both the vegetable and animal lakes in being immediately destroyed by a solution of chlorine.

The lake made from cochineal produced much denser fumes when exposed to fused potash, and afforded a distinct ammoniacal smell. The two modern lakes when burnt in oxygene

did not give stronger signs of inflammation than the ancient. I ascertained the loss of weight this ancient lake suffered by combustion, and found it only $\frac{1}{30}$, and this loss must in great part have depended on the expulsion of water from the clay on which it was fixed. This circumstance induced me to renounce the idea of attempting to determine its nature from the products of its decomposition; which in the case of so small a quantity of matter diffused over so large a quantity of surface could not have afforded unequivocal results.

The durability of this lake, whether vegetable or animal, is a very curious circumstance; but the exterior part which has been exposed to air has suffered.—This durability probably depends in a great measure upon the attractive powers of so large a mass of alumina; for whenever one proportion of a substance is combined with many proportions of another substance, it is very difficult to decompose or detach the one proportion.

From the circumstances which have been noticed respecting this colour, it is impossible to form an opinion whether it is of vegetable or animal origin. If of animal origin, it is most probably the Tyrian or marine purple: and by some comparative experiments on the purple obtained from shell fish the question might perhaps be decided.* It is very probable that the most expensive colour would be employed for orna-

* M. Chaptal considers the lake he found amongst the colours from Pompeii (as I have already mentioned) as of vegetable origin; and he founds his opinion upon the circumstance of its not affording by decomposition the smell peculiar to animal substances: but probably this smell, even if produced by recent purple colouring matter of animal origin, would not belong to colouring matter of 1700 years old. For it is most probably owing merely to albumen or gelatine not essential to the colouring particles, and much more rapidly decomposed.

menting the imperial baths; and it is not impossible that Pliny may have alluded to the palace of the Cæsars when he says " nunc et purpuris in parietes migrantibus, et India conferente fluminum suorum limum, et draconum et elephantorum saniem, nulla nobilis pictura est." Lib. xxxv. Cap. 32.

I have seen no colour of the same tint as this ancient lake in any of the ancient paintings in fresco. The purplish reds in the baths of Titus are mixtures of red ochres and the blues of copper.—In the Aldobrandini picture there is a purple in the garment of the Pronuba, but of an inferior hue; and this purple appears to be a compound mineral colour of the nature of these.—It was not destroyed by solution of chlorine; and when a little of it was exposed to muriatic acid, it rendered the acid yellow, and the remainder yielded a residual blue powder.

VII. *Of the blacks and browns of the Ancients.*

There is one chamber in the baths of Titus of which the ground work is black. I have found several fragments of stucco painted black both in the baths of Titus and in the vineyard above mentioned, and also in some ruins near the Porta del Popolo.—I scraped off some of these colours and submitted them to experiments: they were not acted on by acids or alkalies, they deflagrated with nitre, and had all the properties of pure carbonaceous matter.

I found no blacks, but three different shades of brown in the vase of mixed colours; one was snuff colour, one deep red brown, and the third a dark olive brown. The two first proved to be ochres which had been probably partially calcined; the third contained oxide of manganese, as well as

oxide of iron, and afforded chlorine when acted on by muriatic acid.

All the ancient authors describe the artificial Greek and Roman blacks as carbonaceous, and made either from the powder of charcoal or the decomposition of resin, (a species of lamp black) or from the lees of wine, or from the common soot of wood fires. PLINY mentions the inks of the cuttle fish, but says, " ex his non fit."* Some years ago I examined this substance, and found it a carbonaceous body mixed with gelatine. PLINY speaks of ivory black as invented by Apelles; he says likewise that there is a natural fossil black, and another black prepared from an earth of the colour of sulphur. Probably both these substances are ores of iron and manganese.

That the ancients were acquainted with the ores of manganese is evident from the use made of it in colouring glass. I have examined two specimens of ancient Roman purple glass, both of which were tinged with oxide of manganese.—PLINY speaks of different brown ochres, and particularly of one from Africa, which he names Cicerculum, which probably contained manganese: and THEOPHRASTUS mentions a fossil† which inflamed when oil was poured upon it, a property belonging to no other fossil substance now known but the *black wad*, an ore of manganese, and which is now found in Derbyshire.

The browns in the paintings in the baths of Livia, and in the Aldobrandini picture, are all produced by mixtures of ochres

* i e. the atramentum.

† THEOPHRASTUS says it is like decomposed wood *παρόμοιος ὢν ξύλῳ σαπρῷ*, 12th page of John de Laet's edition.

with blacks. Those in the Aldobrandini picture yield oxide of iron to muriatic acid, but the darker shades were not touched by that acid, nor by solution of alkalies.

VIII. *Of the whites of the Ancients.*

The white colours in the Aldobrandini picture are soluble in acids with effervescence, and have the characters of carbonate of lime.

The principal white in the vase of mixed colours appears to be a very fine chalk. There is another white with a tint of cream colour, which is a fine aluminous clay.

The whites that I have examined from the baths of Titus, and those from other ruins, are all of the same kind.

I have not met with ceruse amongst the ancient colours, though we know from THEOPHRASTUS, VITRUVIUS, and PLINY, that it was a common colour: and VITRUVIUS describes it as made by the action of lead upon vinegar.

Several white clays are mentioned by PLINY as employed in painting, of which the Parætonium was considered as affording the finest colour.

IX. *Of the manner in which the Ancients applied their colours.*

It appears from VITRUVIUS that the colours used in fresco painting were applied moist to the surface of a stucco* formed of powdered marble cemented by lime; he states that the wall or ceiling had three distinct coatings of stucco made of this material, of which the first contained coarse powder of marble, the second the finer powder, and the third the finest

* Lib. vii. Cap. 2, 3, and 4.

powder of all, and that after this the wall was polished before the colour was applied. The stuccos that remain in the ruins of the baths of Titus and Livia are of this kind, and so is the ground of the Aldobrandini picture; they are beautifully white, and almost as hard as marble, and the granular marble of different degrees of fineness may be distinguished in them. This circumstance indeed offers a test of the antiquity of ruins at Rome. In the houses that have been built in the middle and later ages, decomposing lava has been mixed with the calcareous cement instead of granular marble, and the stuccos of these houses are grey or brown, and very coarse in their texture.

Pliny says that purple, orpiment, ceruse, the natural azure, indigo, and the meline white, were injured by application to wet stucco, which is easily explained in the case of orpiment, carbonate of copper, ceruse, and indigo, from their chemical composition.

Vitruvius states that in fresco painting vermilion changed if exposed to light, and he recommends the encaustic process for fixing the colour under this circumstance, namely, laying over it a coat of punic wax, and liquifying the wax so as to make a varnish for the colour.

Pliny describes this process as applied in painting ships; and we know from his authority that several pictures of the great Greek masters were painted in encaustic, and that the different colours were laid on mixed with wax. I have examined several pieces of the painted stuccos found in the different ruins, and likewise the Aldobrandini picture, with a view of ascertaining if any application had been made to fix the colour; but neither by the test of alcohol, nor by heat,

nor by the action of water, could I detect the presence of any wax varnish, or animal or vegetable gluten.

The pot of colours to which I have already referred, found at Pompeii, was blackened by smoke, as if it had been recently on a fire of wood. I thought that this might be owing to some process for dissolving gluten or varnish in the preparation of the colour; but I could detect no substance of this kind mixed with the colouring matter.

PLINY states, that gluten (our glue)* was used in painting with blacks: and this specific mention of its application would induce the belief that it was not employed with other colours, which adhered without difficulty to, and were imbibed by, a surface so polished and well prepared as the Roman stucco; and the lightness of carbonaceous matter alone probably rendered this application necessary.

X. *Some general observations.*

It appears from the facts that have been stated, and the authorities quoted, that the Greek and Roman painters had almost all the same colours as those employed by the great Italian masters at the period of the revival of the arts in Italy. They had indeed the advantage over them in two colours, the Vestorian or Egyptian azure, and the Tyrian or marine purple.

The azure, of which the excellence is proved by its duration for seventeen hundred years, may be easily and cheaply made; I find that fifteen parts by weight of carbonate of soda, twenty parts of powdered opaque flints, and three parts of copper filings strongly heated together for two hours, gave a

* Lib. xxxv. Cap. 25. "Omne atramentum sole perficitur, librarium gummi tectorium glutino admixto."

substance of exactly the same tint, and of nearly the same degree of fusibility, and which, when powdered, produced a fine deep sky blue.

The azure, the red and yellow ochres, and the blacks are the colours that seem not to have changed at all, in the ancient fresco paintings. The vermilion is darker than recently made Dutch cinnabar, and the red lead is inferior in tint to that sold in the shops. The greens in general are dull.

The principle of the composition of the Alexandrian frit is perfect; namely, that of embodying the colour in a composition resembling stone, so as to prevent the escape of elastic matter from it, or the decomposing action of the elements; this is a species of artificial lapis lazuli, the colouring matter of which is naturally inherent in a hard siliceous stone.

It is probable that other coloured frits may be made, and it is worth trying whether the beautiful purple given by oxide of gold, cannot be made useful in painting in a densely tinted glass.

Where frits cannot be employed, metallic combinations which are insoluble in water, and which are saturated with oxygene or some acid matter, it is evident from the proof of a duration of seventeen centuries, are the best pigments. In the red ochres the oxide of iron is fully combined with oxygene, and in the yellow ochres it is combined with oxygene and carbonic acid; and these colours have not changed. The carbonates of copper which contain an oxide and an acid have changed very little.

Massicot and orpiment were probably the least permanent amongst the ancient mineral colours.

Of the colours, the discovery of which is owing to the im-

provements in modern chemistry, the patent yellow is much more durable than any ancient yellow of the same brilliancy; and chromate of lead, an insoluble compound of a metallic acid with a metallic oxide, is a much more beautiful yellow than any possessed by the ancients, and, there is every reason to believe, is quite unalterable.

SCHEELE's green (the arsenite of copper), and the insoluble muriatic combination of copper, will probably be found more unalterable than the ancient greens; and the sulphate of baryta offers a white superior to any possessed by the Greeks and Romans.

I have tried the effect of light and air upon some of the colours formed by the new substance iodine. Its combination with mercury offers a good red, but it is, I think, less beautiful than vermilion, and it appears to change more by the action of light.

Its compound with lead gives a beautiful yellow, little inferior to the chromate of lead; and I possess some of this colour which has been exposed to light and air without alteration for several months.

In many of the figures and ornaments in the outer chambers of the baths of Titus, where only outlines or spots remain, or shades of ochre, it is probable that vegetable or animal colours, such as indigo and the different dyed clays, were used.*

PLINY speaks of the celebrated Greek painters as employing only four colours. "Quatuor coloribus solis immortalia illa opera fecere: ex albis Melino, ex silaceis Attico, ex rubris

* Some excellent pictures have suffered very much in modern times from the same cause; the lakes in the frescos of the Vatican have lost much of the brilliancy which they must have possessed originally. The blues in many pictures of Paul Veronese are become muddy.

Sinopide Pontica, ex nigris atramento, Apelles, Echion, Melanthius, Nicomachus, clarissimi pictores;"* but as far as Apelles and Nicomachus are concerned, this is a mistake: and it is not unlikely that PLINY was misled by an imperfect recollection of a passage in CICERO, who describes the earlier Greek school as using only four colours; but the later Greek painters as perfect masters in all the resources of colouring. "Similis in pictura ratio est: in qua Zeuxim, et Polygnotum, et Timantem, et eorum, qui non sunt usi plus quam quatuor coloribus, formas et lineamenta laudamus: at in Aetione, Nicomacho, Protogene, Apelle, jam perfecta sunt omnia." CICERO, Brutus, seu de claris oratoribus, c. 18. PLINY himself describes with enthusiasm the Venus ἀναδυομένη of Apelles: and in this picture the sea was represented, which required azure.

The great Greek painters, like the most illustrious artists of the Roman and Venetian school, were probably, however, sparing in the use of the more florid tints in historical and moral painting, and produced their effects rather by the contrasts of colouring in those parts of the picture where a deep and uniform tint might be used, than by brilliant drapery.

If red and yellow ochres, blacks and whites, were the colours most employed by Protogenes and Apelles, so they are likewise the colours most employed by RAPHAEL and TITIAN in their best style. The St. John and the Venus, in the tribune of the Gallery at Florence, offer striking examples of pictures in which all the deeper tints are evidently produced by red and yellow ochres, and carbonaceous substances.

As far as colours are concerned, these works are prepared for that immortality which they deserve; but unfortunately

* Lib. xxxv. c. 32.

the oil and the canvas are vegetable materials, and liable to decomposition, and the last is less durable than even the wood on which the Greek artists painted their celebrated pictures.

It is unfortunate that the materials for receiving those works which are worthy of passing down to posterity as eternal monuments of genius, taste, and industry, are not imperishable marble* or stone: and that frits, or unalterable metallic combinations, have not been the only pigments employed by great artists; and that their varnishes have not been sought for amongst the transparent combinations of the earths with water, or amongst the crystalline transparent compounds unalterable in the atmosphere.†

* Copper, it is evident, from the specimens in the ruins of Pompeii, is a very perishable material, and, therefore, even enamels made on copper will yield to time. Canvas, by being impregnated with bitumen, is rendered much more durable, as is evident from the duration of the linen impregnated with bitumen and asphaltum used for infolding the Egyptian mummies.

† The artificial hydrat of alumina will probably be found to be a substance of this kind: possibly the solution of boracic acid in alcohol will form a varnish.—The solution of sulphur in alcohol is likewise worthy of an experiment. Many other similar combinations might be named.

Rome, January 14*th*, 1815.

IX. *On the laws which regulate the polarisation of light by reflexion from transparent bodies. By* David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B.P.R.S.*

Read March 16, 1815.

Dear Sir,

The discovery of the polarisation of light by reflexion, constitutes a memorable epoch in the history of optics; and the name of Malus, who first made known this remarkable property of bodies, will be for ever associated with a branch of science which he had the sole merit of creating. By a few brilliant and comprehensive experiments he established the general fact, that light acquired the same property as one of the pencils formed by double refraction, when it was reflected at a particular angle from the surfaces of all transparent bodies: he found that the angle of incidence at which this property was communicated, was greater in bodies of a high refractive power, and he measured, with considerable accuracy, the polarising angles for glass and water. In order to discover the law which regulated the phenomena, he compared these angles with the refractive and dispersive powers of glass and water, and finding that there was no relation between these properties of transparent bodies, he draws the following general conclusion. "The polarising angle neither "follows the order of the refractive powers, nor that of the

" dispersive forces. It is a property of bodies independent " of the other modes of action which they exercise upon " light."

This premature generalisation of a few imperfectly ascertained facts, is perhaps equalled only by the mistake of Sir Isaac Newton, who pronounced the construction of an achromatic telescope to be incompatible with the known principles of optics. Like Newton, too, Malus himself abandoned the enquiry; and even his learned associates in the Institute, to whom he bequeathed the prosecution of his views, have sought for fame in the investigation of other properties of polarised light.

In the summer of 1811, when my attention was first turned to this subject, I repeated the experiments of Malus, and measured the polarising angles of a great number of transparent bodies. I endeavoured, in vain, to connect these results by some general principle: the measures for *water* and the *precious stones* afforded a surprising coincidence between the indices of refraction and the tangents of the polarising angles; but the results for glass formed an exception, and resisted every method of classification. Disappointed in my expectations, I abandoned the enquiry for more than twelve months, but having occasion to measure the polarising angle of *topaz*, I was astonished at its coincidence with the preceding law, and again attempted to reduce the results obtained from *glass* under the same principle. The piece which I used had two surfaces excellently polished. The polarising angle of one of these surfaces almost exactly accorded with the law of the tangents, but with the other surface there was a deviation of no less than two degrees. Upon examining the cause of this

anomalous result, I found that one of the surfaces had suffered some chemical change, and reflected less light than any other part of the glass. This artificial substance acquires an incrustation, or experiences a decomposition by exposure to the air, which alters its polarising angle without altering its general refractive power. The perplexing anomalies which Bouguer observed in the reflective power of plate glass, were owing to the same cause, and so liable is this substance to these changes, that by the aid of heat alone, I have produced a variation of 9° on the polarising angle of flint glass, and given it the power of acting upon light like the coloured oxides of steel.

Having thus ascertained the cause of the anomalies presented by glass, I compared the various angles which I had measured, and found that they were all represented by the following simple law.

The index of refraction is the tangent of the angle of polarisation.

In the course of last summer, when I had the pleasure of seeing M. Arago, I mentioned to him the relation which I had discovered between the refractive powers, and the tangents of the polarising angles. He informed me, that he had found the polarising angle of air to be 45° or 47°, which being at the very extremity of the scale would afford a good test of the accuracy of the law. Now, if we take the refractive power of air at 1.00031 the polarising angle will be 45° 0′ 32″, a result which agrees most strikingly with the observed angle.

In the following table I have given the polarising angles of eighteen transparent bodies, as determined by experiment, and as deduced from the law of the tangents. I have added in

the *fourth* column the differences between the calculated and observed angles, and in the *fifth* column the calculated angles of polarisation for the second surfaces of the bodies subjected to experiment.

Table containing the calculated and observed polarising angles for various bodies.

Names of the Bodies.	Calculated polarising angles for the *first* surface.			Observed polarising angles for the *first* surface.		Difference between the calculated and observed angles.			Calculated polarising angles for the *second* surface.		
	°	′	″	°	°				°	′	″
Air - - - - - -	45	0	32	45 or	47				44	59	28
Water - - - - -	53	11	0	52°	45′	0°	26′	—	36°	49′	
Fluor spar - - -	55	9	0	54	50	0	19	—	34	51	
Obsidian - - -	56	6	0	56	3	0	3	—	33	54	
Birdlime - - - -	56	40	0	56	46	0	6	+	33	20	
Sulphate of lime -	56	45	0	56	28	0	17	—	33	15	
Rock crystal - -	56	58	0	57	22	0	24	+	33	2	
Opal coloured glass	58	33	0	58	1	0	32	—	31	27	
Topaz - - - - -	58	34	0	58	40	0	6	+	31	26	
Mother of pearl -	58	50	0	58	47	0	3	—	31	10	
Iceland spar - -	58	51	0	58	23	0	28	—	31	9	
Orange coloured glass	59	28	0	59	12	0	16	—	30	32	
Spinelle ruby - -	60	25	0	60	16	0	9	—	29	35	
Zircon - - - - -	63	0	0	63	8	0	8	+	27	0	
Glass of antimony	64	30	0	64	45	0	15	+	25	30	
Sulphur - - - -	63	45	0	64	10	0	25	+	26	15	
Diamond - - -	68	1	0	68	2	0	1	+	21	59	
Chromate of Lead	68	3	0	67	42	0	21	—	21	56	

The coincidence between the calculated and observed angles, as shown in the preceding Table, must appear very remarkable to those who are aware of the difficulty of measuring

correctly the index of refraction for the *mean* refrangible ray, and the still greater difficulty of determining the angle at which the intensity of the evanescent pencil is a minimum. The total amount of the errors in seventeen observations is 259 minutes, which gives an average error of 15′ for each observation. In general the observed angles are less than the calculated angles, the number of negative being to the number of positive differences as 174′ is to 85′.

This circumstance arises from two separate causes, which ought to be carefully kept in view in all experiments on the polarising angles of bodies.

1. In order to illustrate the first of these causes, let us take the case of *Zircon*, in which the intensity of the evanescent pencil is a minimum at 63° of incidence. At 64° the intensity of the pencil which vanishes at 63° is much greater than that of the pencil at 62° on account of its falling more obliquely upon the reflecting surface, and consequently the intensity of that pencil varies more rapidly in passing from 64° to 63° than from 62° to 63°. Hence, in determining the point of minimum intensity, it is more likely, from the way in which the observation must be made, that the observed angle will fall *below* than above the real polarising angle.

2. As the differently coloured rays have different angles of polarisation, and as the most luminous rays of the spectrum have less refractive power than the mean refrangible rays, the observed polarising ought always to be less than the polarising angle for the mean rays. Hence, all the observed angles in the preceding Table ought to be increased by a certain quantity, or, what is the same thing, the index of refraction for the most luminous rays ought to be employed instead

of the mean index of refraction in computing the first column.

The law of the polarisation of light by reflexion being thus experimentally established, we shall now proceed to point out its geometrical consequences, and to arrange, under separate propositions, the new truths to which it leads, as well as those which I have obtained from direct experiment. It will thus be seen, that the subject assumes a scientific form, and that we can calculate *a priori*, the result of every experiment, whether the light is incident upon the first or second surface of transparent bodies, or upon the separating surface of different media, or whether it undergoes a series of successive reflexions in the same plane, or in planes at right angles to each other.

Sect. I. *On the laws of the polarisation of light, by reflexion from the first surfaces of transparent bodies.*

Prop. 1.

When a pencil of light is incident upon a transparent body at an angle, whose tangent is equal to the index of refraction, the reflected portion will be either wholly polarised, or the quantity of polarised light which it contains will be a maximum.

This proposition is a repetition of the general law already established. In *water, glass,* and other bodies, whose refractive power is less than 1.6, almost the whole of the pencil is polarised, at the polarising angle; but in diamond, realgar, chromate of lead, oil of cassia, &c. whose refractive power exceeds 1.6, the whole of the reflected pencil does not suffer polarisation, but the quantity of polarised light is a *maximum* at the angle indicated in the proposition. See Sect. V.

PROP. II.

The differently coloured rays which compose a beam of white light are polarised at angles of reflexion whose tangents are equal to their respective indices of refraction.

This Proposition might have been admitted as a corollary to Prop. I.; but I have established it by the following experiment. A ray of light incident upon *oil of cassia* at an angle of 58° 38′, suffers its maximum polarisation. When the angle is 57° 38′ the *blue* rays predominate in the pencil that approaches to evanescence, while at an angle of 59° 38′ the *red* rays predominate. Hence it follows, that the polarisation of the red light is a maximum at an angle below the mean polarising angle, and the polarisation of the blue light a maximum at an angle, above the mean polarising angle. See Sect. V.

PROP. III.

When the refractive power of any body is infinitely small, its polarising angle will be 45°.

The limit to which the index of refraction constantly approaches is 1.000 which is the tangent of 45°

PROP. IV.

When a pencil of light is polarised by reflexion, the sum of the angles of incidence and refraction is a right angle.

Let MN, (Fig. 1, Pl. VI.) be the reflecting surface, and BA, a ray of light polarised by reflexion in the direction AD, and let AC be the refracted ray. Then since EF, the tangent of the polarising angle BAE, is equal to m, or the index of refraction, we have by the law of the sines, $CL = \frac{BG}{m} = \frac{BG}{EF}$. But from the

similar triangles ABH, AEF, we have AH or BG : HB :: EF : Rad.; and HB $= \frac{BG}{EF}$, consequently CL = HB and the angle BAN = CAK. But EAB + BAN = 90°, hence EAB + CAK = 90°.

Cor. The complement of the polarising angle is equal to the angle of refraction.

Prop. v.

When a ray of light is polarised by reflexion, the reflected ray forms a right angle with the refracted ray.

Since the angles DAM, BAN, CAK (Fig. 1.) are equal to one another, the angle DAC is equal to the right angle MAK: hence the reflected ray AD forms a right angle with the refracted ray AC.

Prop. vi.

If light were polarised simply by the action of the reflecting force, the polarising angle would be 45°.

For when the refracting force is infinitely small, the polarising angle is 45°. The reflecting force is also infinitely small in this case, but any diminution of the reflecting force, however great, does not alter the direction of the reflected ray with respect to the incident ray, or the position of any point or side of a ray with regard to the direction of its motion. It may also be remarked that 45° is the only angle of reflexion at which any point or side of a ray makes a revolution of 90° relative to the direction of its motion. See Prop. VIII, and Cor. 2, of the same Proposition.

Prop. vii.

Every ray of light polarised by reflexion has been acted upon by the refracting force before it has suffered reflexion.

This follows from the light not being polarised at 45°, but at various angles increasing with the refracting force.

Cor. It results from this Proposition, that light suffers a partial refraction before it is reflected; and that the refractive force extends to a greater distance than the reflecting force from the surface of transparent bodies. This result is not only consistent with the most extensive analogies, but affords an explanation of phenomena, which have hitherto been unexplained.

Bouguer, for example, observed that at $87\frac{1}{2}°$ of incidence, a surface of water reflected 614 rays, while glass reflected only 584. Now supposing the light to be refracted by the water and the glass, before it suffers reflexion, the real angle of incidence upon the glass will be only 57° 44′, while the angle of incidence upon the water will be 61° 5′; so that the pencil being incident more obliquely upon the water, ought to be more copiously reflected.

Prop. viii.

When a ray of light is incident at the polarising angle upon any substance whatever, it receives such a change in its direction, by the action of the refracting force, that the real angle of incidence at which it is reflected and polarised is 45°

Let MN, Fig. 2, be the refracting and the reflecting surface, and OP the termination of the sphere of refracting activity. Let a ray RG be incident at G, at the polarising angle, and

let it be refracted into the line GB, before it is reflected from the surface MN.* A part of the ray GB will penetrate the surface MN, and be refracted into the line BF, while another part will be reflected in the direction BA, and again refracted at A into the line AS. Continue SA to C and FB to D. Then since half of the refraction is supposed to be performed before the ray reaches B, and half of it after it penetrates the medium MN, we have BAC = DBC = half the angle of deviation. But by Prop. V, ADB is a right angle, hence ABC is also a right angle, and the angles ABE, GBE, each half a right angle, or 45°.

Cor. 1. At the instant of reflexion, when the refraction at B commences, the refracted ray sets off at right angles to the reflected portion.

Cor. 2. The *real angle of polarisation* is 45°, the effect of the refractive force being merely to bend the ray of light so as to make it suffer reflexion at this particular angle.

Cor. 3. The excess of the angle formed by the incident and the polarised ray, above a right angle, is equal to the angle of deviation. The angle PAB, Fig. 1, which is equal to the angle of deviation OAC, is obviously the excess of DAB above the right angle DAP.

Sect. II. *On the laws of the polarisation of light by reflexion from the second surfaces of transparent bodies.*

When a ray of light is incident upon a parallel plate of

* In order to keep the figure from being complicated, I have supposed the reflexion to take place all at once when the ray reaches the surface MN. The demonstration would have been exactly the same if the ray had been represented as suffering a gradual reflexion in passing through the sphere of reflecting activity.

any transparent body, the image reflected by the first surface is nearly coincident with the image reflected by the second surface, and MALUS observed that they were both polarised at the same time. As the angles at which the rays are incident upon the two surfaces are very different, this result appeared quite inexplicable; but it will be seen from the following Propositions, that the simultaneous polarisation of the two pencils is a necessary consequence of the general law, and derives from that law the most satisfactory explanation.

PROP. IX.

When a pencil of light is incident on the second surface of transparent bodies, at an angle whose co-tangent is equal to the index of refraction, the reflected portion will be either wholly polarised, or the quantity of polarised light which it contains will be a maximum.

As the images formed by the first and second surfaces of a transparent plate are simultaneously polarised, this Proposition is established by the experimental results in the preceding Table.

PROP. X.

The angle of polarisation at the second surface of transparent bodies, is the complement of the angle of polarisation at the first surface.

As the angle of incidence at the second surface is equal to the angle of refraction at the first surface, and as this latter angle is, by the Corollary to Prop. IV. equal to the complement of the angle of polarisation, it follows, that the two polarising angles are complementary to each other.

Prop. xi.

When a ray of light is polarised by reflexion from the second surface of transparent bodies, the reflected ray will form a right angle with the refracted ray.

Let AB, Fig. 3, be a ray incident at the first surface MN, AD the ray polarised at that surface, AC the ray incident at the second surface PQ, and CM the ray polarised at that surface; then if CF be the refracted ray, the angle MCF is a right angle. By Prop. V, DAC is a right angle, and on account of the parallelism of MN, PC, and BA, CF, the angle FCP is equal to DAM, but MCP is equal to MAC, hence the whole MCF is equal to the whole DAC, or a right angle.

Cor. 1. The ray CM, reflected by the second surface, is at right angles to the ray AB incident on the first surface.

Cor. 2. The internal reflected ray CM forms with the external reflected ray AD, an angle equal to the angle of deviation CAO.

Cor. 3. The ray CF emerging from the second surface forms with the first reflected ray, AD an angle equal to the complement of the angle of deviation.

Prop. xii.

When a ray of light is incident at the polarising angle, upon the second surface of transparent bodies, it receives such a change in its direction from the action of the refracting force, that the real angle of incidence, at which it is reflected and polarised, is 45°

By the very same reasoning which was used in Prop. XI,

it may be shown that the angle ABC, *fig.* 4, is a right angle; but BC being a continuation of BG, ABG will also be a right angle, and consequently the angle of incidence EBG will be half a right angle, or 45°.

PROP. XIII.

When a ray of light is incident on the second surface of a transparent body at an angle whose sine is greater than the reciprocal of the index of refraction, or at an angle greater than the angle of total reflexion, the reflected light will consist of two pencils, one of which is polarised in the plane of reflexion, and the other in a plane perpendicular to the plane of reflexion.

The experiments by which I ascertained this singular property were conducted in a manner similar to those of MALUS upon polished metals. A ray of light moving horizontally in the direction of the meridian, after having been polarised in the plane of the horizon, was made to fall upon the second surface of a transparent body facing the south-east or south-west, and inclined at such an angle to the horizon, as to receive the ray near the limit of total reflexion. The polarised ray was depolarised by the action of the second surface, so that the images of the object from which it proceeded, when viewed through a prism of calcareous spar, continued visible in every part of its revolution, an effect which could only be produced by the power of the second surface to form two oppositely polarised images.

When the plane of the second reflexion is either parallel, or perpendicular to the plane in which the ray was originally polarised, the ray will suffer no change by the second reflexion, one of the images formed by a prism of calcareous spar vanishing in every quadrant of its circular motion.

In order to ascertain the relation between the polarising angle at the second surface of transparent bodies, and the angle at which they reflect the whole of the incident pencil, let us make.

The index of refraction	$= m$
Sine of the angle of total reflexion	$\frac{1}{m}$
Cotangent of the polarising angle	m
Tangent of the polarising angle	$\frac{1}{m}$

Since the sine of any angle is always less than the tangent, the polarising angle whose tangent is $\frac{1}{m}$ will always be less than the angle of total reflexion whose sine is $\frac{1}{m}$. The angle of polarisation, therefore, must fall without the limit of total reflexion, but it will gradually approach to that limit as the refractive power increases. When the pencil, however, is incident within the sphere of total reflexion, the quantity of polarised light is so near its maximum, that the experiments can be conducted with almost the same result, as if the polarising angle had exceeded the angle of total reflexion. The only consequence of the difference between the two angles is, that the depolarised image is inferior in point of intensity to the other image.

The following measures for *flint glass* with a refractive power of 1.600, and of *diamond* with a refractive power of 2.80, will show the relations between these two angles.

		°	′
Polarising angle for the second surface of *flint glass*		32	0
Angle at which total reflexion commences	-	38	45
	Difference	6°	45′

Polarising angle for the second surface of *diamond*	21°	59′
Angle at which total reflexion commences -	23	46
Difference	1°	47′

SECT. III. *On the laws of the polarisation of light by reflexion from the separating surfaces of different media.*

Although the attention of MALUS was directed to this branch of the subject, yet he does not appear to have obtained even a single measure of the angles at which light is polarised at the separating surfaces of different media. " After having de-" termined, " he observes," the angles under which polarisa-" tion takes place with respect to different bodies, *water* and " *glass*, for example, I sought for that at which the same " phenomenon would take place at their surface of separation, " when they are in contact, but the law according to which " this last angle depends upon the first two still remains to " be discovered." I have often attempted the same experiment with the same want of success, but besides being unable to determine the *law*, I could never find that there was any maximum angle of polarisation at the common surface of water and glass, when the light was incident from air. It is curious to remark, that MALUS does not say, that such an angle existed although this may be considered as implied in the observation that the law still remains to be determined. Now it is sufficiently singular, as will appear from the following propositions, that there is no angle of incidence at the first surface of the water which will admit the light to be polarised at the common surface of the water and the glass.

PROP. XIV.

When a pencil of light is incident upon the separating surface of two media having different indices of refraction m m′, it will be polarised at an angle whose tangent is equal to the quotient of the greater index of refraction divided by the lesser, or $\frac{m}{m'}$.

This Proposition is a necessary consequence of the general law, and is also deduced from direct experiment.

If we call A the angle whose tangent is equal to $\frac{m}{m'}$, then the corresponding angle at which the pencil is incident from air upon the first surface of the upper medium, or $\alpha = \sin. A \times m$.

In the case of *water* and *glass*, where m is equal to 1.525, and m' to 1.336, we have the polarising angle at the surface of separation, or $A = 48^\circ\, 47'$, and $\sin. A \times m' = 1.0048$, consequently α is greater than 90°. Hence it follows, that when a ray of light is incident upon a parallel plate of water lying upon a plate of glass, there is no angle of incidence upon the first surface of the water at which the ray will suffer polarisation at the separating surface of the two media. The polarisation of the incident pencil increases from 0° to 90°, and is nearly complete at 90°.

When m is equal to 1.508, which is sometimes the case, then $\sin. A \times m' = 1.000$, and $\alpha = 90^\circ$ exactly.

This conclusion was so unexpected that I immediately endeavoured to confirm it by experiment. The result was exactly conformable to the law: the polarisation of the pencil became more and more perfect from 0° to 90° of incidence. Between 80° and 90° the change was scarcely perceptible, owing to the slow variation of the sines, for when the pencil is

incident at 80°, the angle of incidence at the separating surface is 47° 29′, while at an incidence of 90° it is no more than 48° 28′, differing only 59′ from the other.

Prop. XV.

When light is polarised at the separating surface of two media, the sum of the angles of incidence and refraction is a right angle, and the reflected ray forms a right angle with the refracted ray.

This proposition is demonstrated in the same manner as Prop. IV and V, the separating surface producing always the same phenomena as the first surface of any body, whose index of refraction is equal to the quotient of the indices of refraction for the two contiguous bodies.

It would be a waste of time to extend the application of the general law to other cases where the reflecting surfaces are inclined at different angles, or where the incident pencil traverses a number of different media, and receives particular changes at each successive reflexion. We shall, therefore, go on to another branch of enquiry, and consider the laws which regulate the phenomena when a pencil is polarised by several successive reflexions, a subject to which Malus has not even alluded.

Sect. IV. *On the law of the polarisation of light by successive reflexions.*

Prop. xvi.

When a ray of light is incident at any angle except a right angle upon the surface of a transparent body, a certain portion of the reflected light is completely polarised, while the remaining portion has suffered a physical change, or has acquired, in various degrees, a character approaching to complete polarisation.

This proposition has been established by direct experiments made with glass, whose polarising angle is 56° 45′.

If a pencil of light is reflected from glass at an angle of 62° 30′, or 50° 20′, i. e. either above or below the polarising angle, the portion of light which is not completely polarised, has so far received this character, that it will be completely polarised by a second reflexion at the same angle, whereas had it been absolutely unpolarised light, it could not have been polarised at any angle different from 56° 45′, the real angle of polarisation.

In like manner *three* reflexions at an angle of 65° 33′ or 46° 30′, or *four* reflections at an angle of 67° 33′, or 43° 51′ will polarise the whole pencil, while at angles above 82° or below 18° more than 100 reflexions are necessary to produce complete polarisation.

The truth of the proposition for the transmitted rays is established by the experiments which I have already published on the polarisation of light by oblique refraction.* If a pencil

* See *Phil. Trans.* 1814, Part I. p. 219.

of light is polarised by twenty-four plates at an angle of 60°, then it is obvious that twelve plates will not polarise the whole pencil at the same angle.

Let us suppose that the portion not polarised amounts to twenty rays out of a hundred. Then, if these twenty rays were absolutely unpolarised, and in the same state as direct light, they would require to pass through twenty-four plates at an angle of 60°, in order to be polarised: but the experiments show that they require only to pass through other twelve plates at that angle. It therefore follows, that the twenty rays have been half polarised by the first twelve plates, and the polarisation completed by the other twelve. Hence we see the mistake of Malus, who observes, that the light transmitted obliquely through glass consists 1st. of a quantity of polarised light, and " 2d. *of another portion not modified,* " *and which preserves the characters of direct light.*"

Prop. XVII.

If a ray of light is partly polarised by reflexion at any angle, it will be more and more polarised by every successive reflexion in the same plane, till its polarisation is complete, whether the reflexions are made at angles all above or all below the polarising angle, or at angles some of which are above and some below the polarising angle.

This proposition is deduced from numerous experiments which may be easily repeated. It is extremely difficult, however, on account of the rapid attenuation of the light when it has undergone a few reflexions from glass, to determine satisfactorily the relation between the number of reflexions and the angles of incidence at which they polarise a pencil of light. The experiments which I have made are represented by the

following law, A, *a* being the angles of incidence above and below the polarising angle, *m* the index of refraction, and N, *n* the number of reflexions above or below the polarising angle.

$$\text{Tang. A} = m \times \sqrt[3]{\text{N}}$$

when the angle of incidence is *greater* than the polarising angle for one reflexion, and

$$\text{Tang. } a = \frac{m}{\sqrt[3]{n}}$$

when the angle of incidence is less than the polarising angle for one reflexion. Hence we have

$$\text{N} = \left(\frac{\text{tang. A}}{m}\right)^3 \text{ and}$$

$$n = \left(\frac{m}{\text{tang. } a}\right)^3$$

When the successive reflections are made at different angles A, A′, A″ above the polarising angle, or *a*, *a*′, *a*″ below the polarising angle, the pencil will be just polarised when

$$\left(\frac{1}{\frac{\text{tang. A}}{m}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}'}{m}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}''}{m}}\right)^3 = 1. \text{ or when}$$

$$\left(\frac{1}{\frac{m}{\text{tang. } a}}\right)^3 + \left(\frac{1}{\frac{m}{\text{tang. } a'}}\right)^3 + \left(\frac{1}{\frac{m}{\text{tang. } a''}}\right)^3 = 1.$$

When some of the reflexions are made above, and some below the polarising angle, at the angles A, *a*, *a*′ A″ for example, then the pencil will be polarised when

$$\left(\frac{1}{\frac{\text{tang. A}}{m}}\right)^3 + \left(\frac{1}{\frac{m}{\text{tang. } a}}\right)^3 + \left(\frac{1}{\frac{m}{\text{tang. } a'}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}''}{m}}\right)^3 = 1.$$

When the refractive power is infinitely small, which is nearly the case in *air*, and the *gases*, we have $m = 1.000$ and $\text{N} = (\text{tang. A})^3$, $n = \left(\frac{1}{\text{tang. } a}\right)^3$. Hence when $\text{N} = n$, cotang. $a = \text{tang. A}$, and therefore *a* will in this case be the complement

of A, the same effect being produced at angles equidistant from the maximum polarising angle.

In order to apply the formulæ with facility in ascertaining the number of reflexions necessary to polarise a pencil of light, I have calculated the following Table containing values of A and *a* for various values of N and *n*, the reflexions being supposed to be made from glass, whose index of refraction is equal to 1.525.

Table showing the angles at which a pencil of light is polarised by any number of reflexions at the same angle.

When the angles are *greater* than the maximum polarising angle, or 56° 45′.			When the angles are *less* than the maximum polarising angle, or 56° 45′.		
Number of reflexions necessary to polarise the incident light.	Angles at which the incident light is wholly polarised by the number of reflexions in Col. 1.		Number of reflexions necessary to polarise the incident light.	Angles at which the incident light is wholly polarised by the number of reflexions in Col. 3.	
	°	′		°	′
1	56	45	1	56	45
2	62	30	2	50	26
3	65	33	3	46	30
4	67	33	4	43	51
5	69	1	5	41	43
6	70	9	6	40	0
7	71	5	7	38	33
8	71	51	8	37	20
9	72	30	9	36	15
10	73	4	10	35	18
27	77	40	27	26	39
64	80	41	64	20	52
100	81	57	100	18	11
125	82	32	125	16	58
1000	86	15	1000	8	46

Let it be required, for example, to ascertain what effect will be produced upon a ray of light by four reflexions at the following angles.

		°	′
Above the polarising angle	-	77	40
		70	9
Below the polarising angle	- -	50	26
		35	18

The values of N and n in the Table corresponding to these angles are, 27, 6, 2, 10, and 64, and therefore we have

$$\frac{1}{2} + \frac{1}{6} + \frac{1}{2} + \frac{1}{10} = \frac{2604}{3240},$$

which being less than 1, the ray will not be polarised, but will require another reflexion either at 69° 1′ or 41° 43′, for the values of N and n corresponding to these angles are both 5 and $\frac{1}{5} + \frac{2604}{3240} = \frac{1626}{1620} = 1$ very nearly.

The formulæ in the preceding proposition are equally applicable to the second surfaces of transparent bodies, and to the separating surfaces of different media, $\frac{1}{m}$, and $\frac{m}{m'}$ being in these cases substituted in place of m.

PROP. XVIII.

When a ray of light is once completely polarised, its polarisation will suffer no change, and the ray will preserve all its optical properties after any number of reflexions at any angle in the plane in which it was polarised, or after any number of refractions in a plane at right angles to that in which it was polarised.

If a ray is polarised by reflexion in the plane of the horizon, and is afterwards reflected at various angles in the plane of the horizon, one of the images will always vanish in the same position of the prism, as if it had not suffered a second

reflection. The same thing will happen if the direct ray is transmitted through a bundle of glass plates in which the plane of refraction is perpendicular to the horizon.

Prop. xix.

When a polarised ray is incident at any angle upon a transparent body, in a plane at right angles to the plane of its primitive polarisation, a portion of the ray will lose its property of being reflected, and will entirely penetrate the transparent body. This portion of light, which has lost its reflexibility, increases as the angle of incidence approaches to the polarising angle, when it becomes a maximum.

A part of this Proposition constitutes one of the beautiful discoveries of Malus, who found that at the polarising angle the second plate of glass " would no longer reflect a single " particle of light, either from its first or second surface."

The rest of the Proposition I have established by various experiments. In realgar, diamond, and oil of cassia, and in substances whose refractive power exceeds 1.600, the portion of light which *suffers reflexion* at the polarising angle is very considerable, and it will be seen from the Propositions in Sect. V., that if strong lights are used, there are no circumstances under which *every particle* of a beam of white light can lose its reflexibility.

Prop. xx.

If the reflecting plane upon which the polarised ray is received, is made to deviate in the slightest degree from the position which deprives the maximum portion of the ray of its reflexibility, a part of the light that had formerly lost its reflexibility will now suffer reflexion, and will be polarised in the plane of the second reflexion, *whereas before the deviation took place, this portion of light was polarised in the plane of the first reflexion.*

When the plane of the second reflexion is perpendicular to the plane of primitive polarisation, every particle of the ray that suffers reflexion will be polarised in the last of these planes, but when the least deviation takes place, a part of the polarised ray is depolarised, so that it receives its character from the second reflexion, and is polarised in the plane of that reflexion. It is very interesting to observe two such opposite effects produced by the most minute change in the position of the second reflecting plane.

Prop. xxi.

If a ray of light reflected from a transparent body at any angle, excepting the polarising angle, is reflected from another body in a plane at right angles to that of its first reflexion, the reflected portion will be polarised by the second reflexion in the same manner, and at the same angle as if it had been direct light.

This proposition is deduced from experiment. The portion of light that is polarised at the first reflexion, will lose its reflexibility at the second reflexion, in proportion as the angle of reflexion approaches to the polarising angle.

PROP. XXII.

When a ray of light polarised by reflexion, is incident at any angle on the surface of a transparent body, so that the plane of the second reflexion is at right angles to the plane of the first reflexion, and suffers successive reflexions in the plane of the second reflexion, it will lose its reflexibility when it has undergone that number of reflexions which would have been necessary to polarise it, had it been direct light.

This result was at first deduced a priori from Prop. XVII. and XIX., and was afterwards established by experiment. The number of reflexions may be determined by the formulæ in Prop. XVII.

PROP. XXIII.

When a beam of light is emitted by the sun, or by any other body which does not shine by reflected light, the particles which compose it are in every state of POSITIVE *and* NEGATIVE *polarisation from particles completely polarised to particles not polarised at all.*

This Proposition is an expression of the experimental results in Prop. XVI. and XVII., and may be illustrated in the following manner, the terms *positive* and *negative* polarisation being employed to denote the two kinds of polarisation by reflexion and refraction at the polarising angle, or by reflexion in two opposite planes. A ray of direct light, before it is incident upon glass, may therefore be represented as consisting of a number of particles p, p, &c. of the following character.

$$+\frac{p}{0^\circ}, \frac{p}{1^\circ}, \frac{p}{2^\circ}, \frac{p}{3^\circ}, \frac{p}{4^\circ}, \frac{p}{5^\circ}, \frac{p}{6^\circ} \cdots\cdots \frac{p}{56^\circ\ 45'}$$

$$-\frac{p}{0^\circ}, \frac{p}{1^\circ}, \frac{p}{2^\circ}, \frac{p}{3^\circ}, \frac{p}{4^\circ}, \frac{p}{5^\circ}, \frac{p}{6^\circ} \cdots\cdots \frac{p}{56^\circ\ 45'}$$

The particle $+\frac{p}{0^\circ}$ will represent a particle so completely polarised in a positive manner, that it will be polarised by reflexion at 0° of incidence; $-\frac{p}{0^\circ}$ will represent a particle so completely polarised in a negative manner that it will be polarised by reflexion in an opposite plane at 0° of incidence; $+\frac{p}{1^\circ}$ a particle so far polarised that it will require only a reflexion at 1° of incidence to complete its polarisation, and so on with all the other particles till we come to $\frac{p}{56^\circ\, 45'}$, which is a particle of direct light so completely unpolarised that it requires to be reflected at the maximum polarising angle before it can suffer complete polarisation.

This peculiar state of the rays before they fall upon a transparent body might have been deduced *a priori* by considering that when a mass of particles is projected from a self-luminous body, the different sides of the rays, or poles of the luminous particles must have every possible position relative to the direction of their motion, which is the state described in the Proposition. If we break a tourmaline, for example, into a number of fragments, there will be a positive and a negative electrical pole in every possible direction, and a mass of moving tourmalines will have, nearly, the same relation to the tourmaline itself in which all the axes are regularly arranged, as a beam of direct has to a beam of polarised light.

Corollary 1. A beam of light that has suffered reflexion at any angle above 0°, will be in a state which may be represented in the following manner, the angle of incidence being supposed to be 4°.

$$\text{Direct light} \quad +\frac{p}{0^\circ}, \frac{p}{1^\circ}, \frac{p}{2^\circ}, \frac{p}{3^\circ}, \frac{p}{4^\circ}, \frac{p}{5^\circ} \frac{p}{6^\circ} \ldots \frac{p}{56^\circ\, 45'}$$

$$\left.\begin{array}{l}\text{Light reflected}\\ \text{at an angle of } 4^\circ\end{array}\right\} +\frac{p}{0^\circ}, \frac{p}{0^\circ}, \frac{p}{0^\circ}, \frac{p}{0^\circ}, \frac{p}{0^\circ}, \frac{p}{z\,5^\circ}, \frac{p}{z\,6^\circ} \ldots \frac{p}{z\,(56^\circ\, 45')}.$$

The negative part of the reflected beam may be represented in a similar manner. The particle of direct light $\frac{p}{0^\circ}$ being already polarised, will suffer no change by the reflexion (Prop. XVIII.) The particle $\frac{p}{1^\circ}$ being susceptible of polarisation by reflexion at an angle of 1° will also be polarised at any angle above 1°, and will therefore come into the state of $\frac{p}{0^\circ}$. In like manner the particles $\frac{p}{2^\circ}$, $\frac{p}{3^\circ}$, $\frac{p}{4^\circ}$ will all be polarised, and assume the state represented by $\frac{p}{0^\circ}$. The particle $\frac{p}{5^\circ}$ being susceptible of polarisation only at an angle of 5° or more will not be polarised at 4°, but will be brought into a state very near that of polarised light. It will therefore be represented by $\frac{p}{z\,5^\circ}$, z being a fractional coefficient, always less than 1, to be determined by the formulæ in Prop. XVII. For the same reason all the other particles will be brought into a state nearer that of perfect polarisation, and will be represented by $\frac{p}{z\,6^\circ}$ and $\frac{p}{z\,(56^\circ\, 45')}$.

Cor. 2. In general, all the particles of a direct beam of light whose denominator is equal to or less than the angle of incidence will be brought by reflexion into the state of $\frac{p}{0^\circ}$, while all the particles whose denominator exceeds the angle of incidence will be brought into a state which may be found by multiplying their state in the direct beam by $\frac{1}{z}$, z being determined by Prop. XVII.

Sect. V. *On the nature and origin of the apparently unpolarised light which exists at the maximum polarising angle.*

I have already shown in a former paper,* that in substances of a high refractive power, such as *realgar* and *diamond*, there is a great quantity of apparently unpolarised light reflected at the polarising angle, so that the image which would have vanished by the application of calcareous spar, had the reflexion been made from *water* or *crown glass*, possessed in these cases a considerable brilliancy. The comparative intensity of the light of this image is indeed so great, that only a very small portion of the incident light seems to be polarised. I was at first much surprised at witnessing this phenomenon, as I had been led to believe from the first Memoir of Malus, that one of the pencils formed by calcareous spar vanished when the light was reflected from all other bodies as well as from *water* and *glass*. There is some reason to think, however, that Malus afterwards observed the same fact, for in a subsequent Memoir, he makes use of the term *maximum polarising angle*, in which the knowledge of it seems to be implied.

The extreme difficulty of accounting for such an unexpected phenomenon, probably deterred him from even mentioning the subject in any of his Memoirs; and I am not ashamed to avow that the investigation of this point alone has cost me more labour than any other branch of the polarisation of light. The existence of a quantity of apparently unpolarised light, in a pencil reflected at the polarising angle, appeared completely paradoxical, and it was obvious that no satisfactory generalisation of the phenomena could be given while this difficulty remained unsolved.

* See Phil. Trans. for 1814, Part I. p. 230.

I at first imagined that bodies with a high refractive power approached to the metals in their mode of action upon light, but this conjecture was refuted by experiments which showed that the pencil did not consist of two oppositely polarised portions. When I discovered the law of successive reflexions, as stated in Prop. XVI. and XVII., the difficulty of explaining the phenomenon seemed to increase. What MALUS would have called the unpolarised portion of a beam of light, reflected at 62° from glass, was now shown to be so far polarised, that its polarisation was completed by a second reflexion at the same angle, so that it became still more improbable that unpolarised light could exist at the polarising angle itself. All these difficulties, however, were immediately removed by the discovery of the law of the tangents, and of the polarisation of the differently coloured rays at angles of incidence depending on their respective indices of refraction. The explanation which now suggested itself was confirmed by experiment, and I was thus led after much fruitless investigation to the results expressed in the following Propositions.

PROP. XXIV.

If a pencil of white light is incident at the maximum polarising angle upon any transparent body whatever, a portion of the reflected pencil, consisting of the mean refrangible rays, will be completely polarised, while another portion of the beam, consisting of the blue and red rays, will not be completely polarised, and will therefore not vanish when the image from which the light proceeds is examined with a prism of calcareous spar.

It is obvious from Prop. II. that all the rays which compose a beam of white light cannot be polarised at the same angle

of incidence. When the pencil is incident at the maximum polarising angle, or at an angle whose tangent is equal to the index of refraction for the mean refrangible rays, these rays alone will be polarised. Neither the *red* rays, which are incident at an angle above their polarising angle, nor the blue rays which are incident at an angle below their polarising angle, will be completely polarised; and when the reflected pencil which contains them is viewed through a doubly refracting crystal, the *mean* refrangible rays will vanish, while the *red* and *blue* rays will compose a beam nearly white, and will not vanish, in consequence of its not being completely polarised.

PROP. XXV.

If a pencil of WHITE *light polarised by reflexion is incident at the polarising angle upon any transparent surface, so that the plane of the second reflexion is at right angles to the plane of its primitive polarisation, a portion of the pencil consisting of the mean refrangible rays will lose its reflexibility, and will entirely penetrate the second surface, while another portion of the beam, composed of the blue and red rays, will not lose its reflexibility, but will suffer reflexion and refraction like ordinary light.*

This proposition founded also on experiment may be proved by the same reasoning as the preceding, for since the angle at which polarised rays lose their reflexibility is the same as the angle at which they are polarised, only one set of the rays which compose a white beam can lose their reflexibility at the same angle.

Prop. xxvi.

The imperfectly polarised portion of light described in Prop. XXIV. and the portion which does not lose its reflexibility as described in Prop. XXV., increase with the dispersive and the refractive power of the reflecting surface, so that in substances where the dispersive and refractive forces are very great, these portions constitute in the one case almost the whole of the reflected pencil, and in the other almost the whole of the pencil that would have been reflected under ordinary circumstances.

When the dispersive power of the reflecting surface is so high as to throw the blue and red rays to a great distance from the mean ray, the quantity of polarised light at the mean polarising angle must be very small, and must obviously diminish as the dispersive power increases, the quantity of imperfectly polarised light consisting of the blue and red rays increasing in the same proportion. When the refractive power is high, the polarising angle increases, and the quantity of reflected light becomes very great, being, in the case of diamond, about one half of the incident beam. Hence in rock crystal, which has a higher refractive power, and a lower dispersive power than water, the image does not wholly vanish at the polarising angle.

PROP. XXVII.

If a pencil of HOMOGENEOUS *or coloured light is incident upon any transparent body at an angle whose tangent is equal to the index of its refraction, every ray of the reflected pencil will be completely polarised in the plane of reflexion.*

This proposition is a necessary consequence of those which precede it; and I have also established it by direct experiments upon diamond and realgar.

PROP. XXVIII.

If a pencil of HOMOGENEOUS *or coloured light is incident under the circumstances described in Prop. XXV., every ray of it will lose its reflexibility.*

This proposition, which is also deducible from those which precede it, has been established by experiment.

PROP. XXIX.

If a beam of WHITE *light suffers more than one reflexion, every ray of it will be completely polarised when the angles of incidence are of such a magnitude that the sum of the terms of the formulæ given under Prop. XVII. is equal to* 1, *the index of refraction for the extreme red ray being substituted in place of* m *if the angles are above the polarising angle, and the index of refraction for the extreme blue ray if the angles are below the polarising angle.*

This Proposition is manifestly deducible from Prop. XVII. compared with Prop. XXVII. Calling *dm* the part of the

whole refraction to which the dispersion, or the distance between the extreme rays is equal, the formulæ will become

$$\left(\frac{1}{\frac{\text{tang. A.}}{m-\frac{1}{2}dm}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}'}{m-\frac{1}{2}dm}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}''}{m-\frac{1}{2}dm}}\right)^3 = 1$$

$$\left(\frac{1}{\frac{m+\frac{1}{2}dm}{\text{tang. } a}}\right)^3 + \left(\frac{1}{\frac{m+\frac{1}{2}dm}{\text{tang. } a'}}\right)^3 + \left(\frac{1}{\frac{m+\frac{1}{2}dm}{\text{tang. } a''}}\right)^3 = 1$$

If the angles are partly above, and partly below the polarising angle, for example, at the angles A, a, a' A$''$ then the formula will become

$$\left(\frac{1}{\frac{\text{tang A}}{m-\frac{1}{2}dm}}\right)^3 + \left(\frac{1}{\frac{m+\frac{1}{2}dm}{\text{tang. } a}}\right)^3 + \left(\frac{1}{\frac{m+\frac{1}{2}dm}{\text{tang. } a'}}\right)^3 + \left(\frac{1}{\frac{\text{tang. A}''}{m-\frac{1}{2}dm}}\right)^3 = 1$$

Scholium.

I have determined the values of *dm* for 151 different substances, and have published a Table containing 137 of these in my *Treatise on New Philosophical Instruments*, p. 315. The value of *m* or the mean index of refraction, was found in the common way by measuring the angle of deviation produced by a prism of the substance under examination. The values of *dm* were computed, from measures taken with a new instrument, by means of a formula investigated by Boscovich, and used in the reduction of all his valuable experiments. This formula requires that the ray should be incident perpendicularly upon the first surface, but it will be found in practice that the dispersion of the prism under examination is equally corrected by the standard prism, when the ray is incident several degrees on either side of the perpendicular.

I have thus endeavoured as briefly as possible, and perhaps

more briefly than a new subject required, to give an account of the experiments and reasonings by which I have established the laws of the polarisation of light by reflexion from transparent bodies. These experiments have been extended to other branches of this subject, and in subsequent Memoirs I shall take the liberty of soliciting your attention to the laws which regulate the polarisation of light by *transmission through uncrystallized plates;*—by *reflexion from metallic and oxidated surfaces*; and by the *separation of light into two pencils by the action* of regularly crystallized bodies. In the investigation of the properties of metallic and oxidated surfaces, my experiments have been attended with the most successful results. I have discovered that the beautiful *complementary colours* produced by the action of crystalline bodies upon polarised light, are exhibited under singular circumstances by reflexions from *silver* and *gold*, and to a certain degree from *other metals;*—and that some metallic bodies have the power of polarising a beam of light in the plane of incidence by six or seven successive reflexions, while other metals are not able to polarise it even after twenty or thirty successive reflexions.

In these enquiries I have made use of no hypothetical assumptions. In imitation of Malus, the language of theory has been occasionally employed, but the terms thus introduced are merely expressive of experimental results, and enable us to avoid frequent and perplexing circumlocutions. The science of physical optics is not yet in such a state as to authorise the construction of a new nomenclature. When discovery shall have accumulated a greater number of facts, and connected them together by general laws, we may then safely begin to impose better names, and to speculate respecting the

Fig. 1.

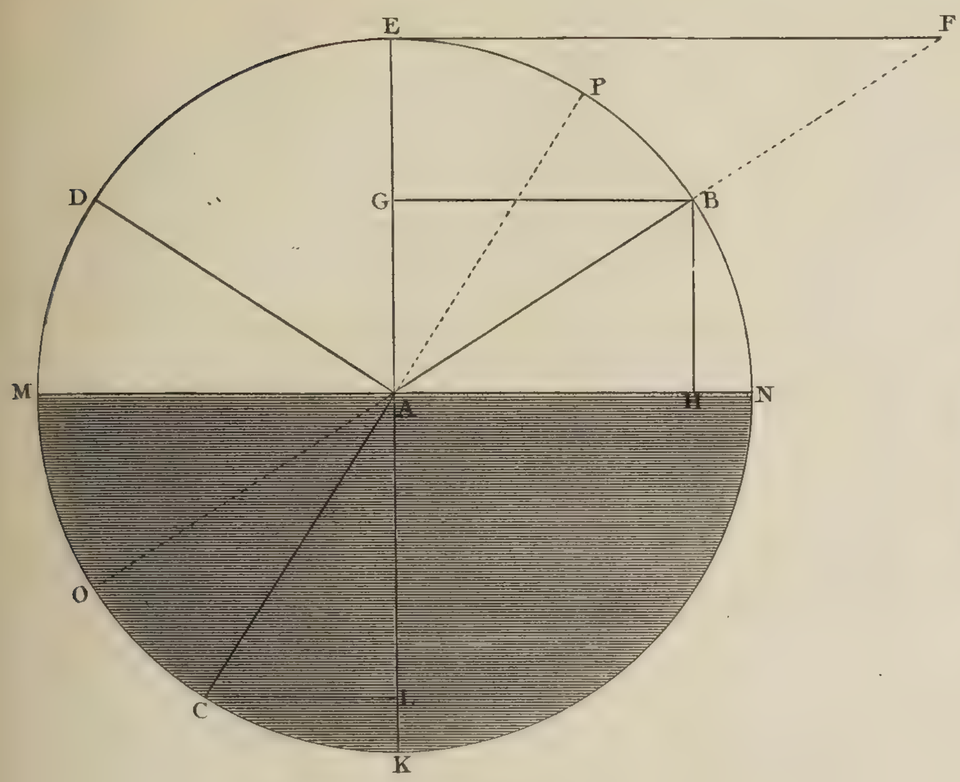


Fig. 3.

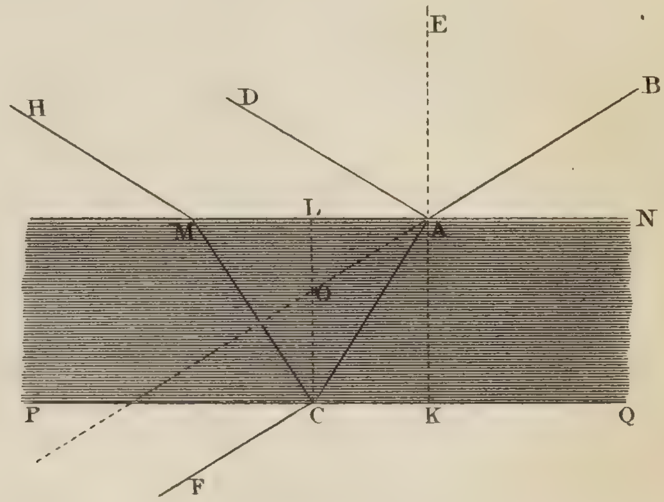


Fig. 2.

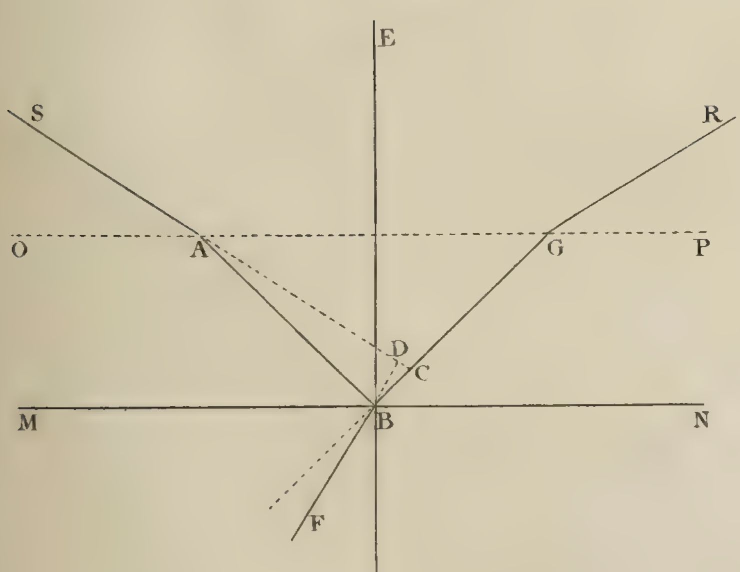
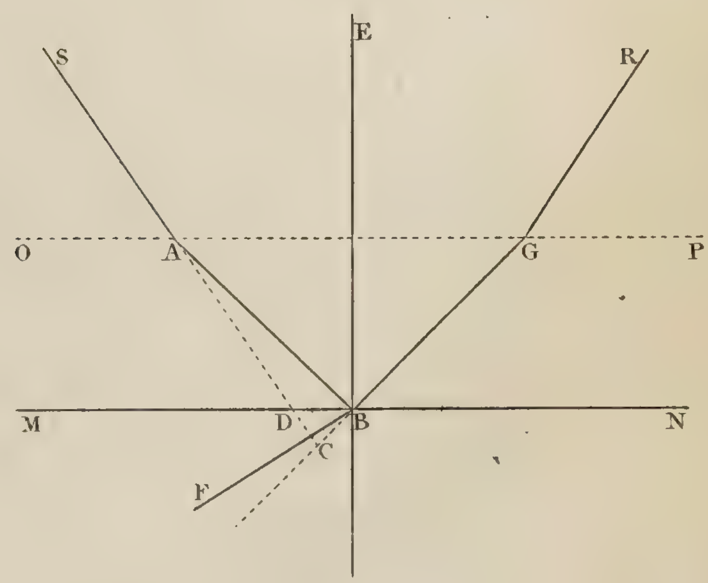


Fig. 4.



J. Basire sc.

cause of those wonderful phenomena which light exhibits under all its various modifications.

In the preceding pages, I have more than once had occasion to establish conclusions opposite to those which Malus had deduced from less numerous experiments; and indeed the whole of this paper is founded on relations which he believed to have no existence. In differing, however, from this eminent philosopher, I trust I have always done it with that respect which it is impossible not to feel for his character and labours. It has fallen to the lot of few to enrich science with so many new and striking discoveries, and if he has failed in pursuing them through all their consequences, we must ascribe it to the limited interval which he was allowed to devote to science, and to the influence of that cruel disease which terminated so prematurely his short but brilliant career. Those, who without repeating his experiments endeavoured during his life to depreciate his labours, are alone capable of wounding his memory. Those who, like him, have pursued science under the oppression of bodily suffering;—who have been instructed and delighted with his discoveries, and who have patiently followed him in the path of research, will feel it their truest pride to do justice to his memory, and will never be able to review his labours without mingling their sorrow with their admiration.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, February 11, 1815.

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METEOROLOGICAL JOURNAL,

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PRESIDENT AND COUNCIL.

MDCCCXV. a

METEOROLOGICAL JOURNAL

for January, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	26	41	30,20	76	Rain this Month 1,122 Inches.	E	1	Thick fog.
	3	0	30	44	30,12	75		E	0	Thick fog.
2	8	0	28	40	29,89	77		E	0,1	Foggy.
	3	0	33	39	29,83	77		SW	1	Cloudy.
3	8	0	30	40	29,73	77		W	0	Foggy.
	3	0	31	40	29,67	78		W	1	Foggy.
4	8	0	30	40	29,42	77		N	1	Snow.
	3	0	29	42	29,30	74		NE	1	Cloudy.
5	8	0	31	39	29,18	77		N	1,2	Cloudy.
	3	0	33	43	29,13	80		E	2	Snow.
6	8	0	33	41	29,09	78		N	1	Cloudy.
	3	0	32	43	29,32	76		NE	1	Cloudy.
7	8	0	24	39	29,61	74		E	1	Cloudy.
	3	0	27	42	29,63	72		NNE	1	Fine.
8	8	0	22	37	29,63	74		N	1	Fine.
	3	0	30	41	29,63	77		N	1	Cloudy.
9	8	0	24	36	29,62	76		E	1	Cloudy.
	3	0	27	37	29,65	75		NE	1	Fine.
10	8	0	17	33	29,77	72		E	1	Foggy.
	3	0	26	38	29,82	72		E	1	Cloudy.
11	8	0	22	29	29,88	69		ESE	1,2	Cloudy.
	3	0	24	36	29,85	69		E	1	Cloudy.
12	8	0	21	33	29,54	69		N	1	Fine, rather hazy.
	3	0	26	38	29,48	68		NW	1	Cloudy.
13	8	0	25	34	29,90	73		N	1	Hazy.
	3	0	30	37	30,06	70		E	1,2	Fine.
14	8	0	22	33	30,01	68		E	1,2	Cloudy.
	3	0	24	37	29,87	68		E	2	Cloudy.
15	8	0	25	34	29,61	70		E	1	Cloudy.
	3	0	28	38	29,57	70		N	1	Cloudy.
16	8	0	27	35	29,39	75		NE	1	Cloudy.
	3	0	31	35	29,23	77		NE	1	Cloudy.

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for January, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	24	34	29,62	71	Rain this Month 1,122 Inches.	N	1	Fine.
	3	0	27	37	29,70	62		NE	1	Cloudy.
18	8	0	31	35	29,12	79		E	1	Snow. Much snow in the night.
	3	0	32	40	29,10	79		E	1	Snow.
19	8	0	32	35	29,09	79		E	1	Cloudy.
	3	0	32	40	29,07	79		E	2	Snow.
20	8	0	29	37	29,37	75		N	2	Snow or sleet.
	3	0	31	40	29,58	70		N		Cloudy.
21	8	0	21	37	29,77	70		W	1	Snow.
	3	0	27	41	29,78	68		W	1	Fine.
22	8	0	22	36	29,75	70		NW	1	Cloudy.
	3	0	28	40	29,74	70		N	2	Fine.
23	8	0	24	35	29,84	72		N	1	Fine.
	3	0	31	37	29,75	71		NE	1	Cloudy.
24	8	0	26	35	29,78	72		N	1	Cloudy.
	3	0	28	39	29,81	72		N	1	Fine.
25	8	0	27	37	29,88	70		NW	1	Foggy.
	3	0	30	40	29,88	65		W	1	Cloudy.
26	8	0	32	37	29,64	73		SW	1	Snow.
	3	0	34	41	29,49	76		S	1	Cloudy.
27	8	0	33	38	29,23	79		S	1	Cloudy.
	3	0	37	38	29,17	75		S	1	Cloudy.
28	8	0	35	42	29,16	79		W	1	Cloudy.
	3	0	36	44	29,24	67		W	1	Cloudy.
29	8	0	33	42	28,66	78		SE	2	Cloudy.
	3	0	40	47	28,32	79		SW	2	Rain.
30	8	0	33	43	28,90	72		W	0,1	Hazy.
	3	0	38	42	29,11	69		W	1	Cloudy.
31	8	0	29	40	29,30	73		W	1	Fine.
	3	0	36	45	29,41	67		W	1	Cloudy.

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1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Feb. 1	8	0	32	41	29,83	67	Rain this Month 0.528 Inches.	W	1	Fine.
	3	0	37	47	30,00	64		SW	1	Fine.
2	8	0	35	42	29,93	75		N	1	Cloudy.
	3	0	38	47	29,98	70		N	1	Cloudy.
3	8	0	28	41	30,06	72		NW	1	Cloudy.
	3	0	33	46	30,01	68		N	1	Fine.
4	8	0	25	42	30,05	72		NE	1	Cloudy.
	3	0	33	47	30,12	66		E	1	Fine.
5	8	0	29	41	30,14	70		S	1	Cloudy.
	3	0	35	46	29,97	76		W	1	Rain.
6	8	0	39	43	29,72	78		W	1	Cloudy.
	3	0	42	44	29,57	72		NW	1	Fine.
7	8	0	36	42	29,62	70		W	1	Cloudy.
	3	0	41	46	29,68	67		W	1	Cloudy.
8	8	0	40	43	29,54	78		W	1	Rain.
	3	0	47	49	29,38	68		N	1	Fine.
9	8	0	38	46	29,67	71		W	1	Cloudy.
	3	0	46	50	29,82	69		NW	1	Cloudy.
10	8	0	47	48	29,99	78		SW	1	Rain.
	3	0	47	52	30,03	74		SW	1	Cloudy.
11	8	0	42	50	30,04	76		S	2	Cloudy.
	3	0	48	55	29,98	69		S	1	Cloudy.
12	8	0	42	51	29,99	73		S	1	Cloudy.
	3	0	47	52	30,00	75		S	1	Cloudy.
13	8	0	42	52	29,96	76		S	1	Cloudy.
	3	0	45	52	29,97	72		S	1	Cloudy.
14	8	0	38	50	29,93	75		E	1	Cloudy.
	3	0	38	52	30,01	68		E	1	Cloudy.
15	8	0	39	48	30,10	71		NE	1	Hazy.
	3	0	40	53	30,11	69		NE	1	Fine.
16	8	0	33	47	30,14	69		E	1	Cloudy.
	3	0	42	52	30,23	69		N	1	Fine.

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for February 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.		Winds. Points.	Winds. Str.	Weather.
Feb. 17	8	0	33	48	30,40	° 69	Rain this Month 0,528 Inches.	E	1	Cloudy.
	3	0	34	50	30,46	64		N	1	Fine.
18	8	0	24	44	30,42	70		W	1	Fine.
	3	0	40	50	30,32	67		W	1	Fine.
19	8	0	37	46	30,29	75		N	1	Rain.
	3	0	39	49	30,28	71		N	1	Cloudy.
20	8	0	27	44	30,44	69		N	1	Cloudy.
	3	0	38	48	30,43	64		N	1	Fine.
21	8	0	26	42	30,36	70		W	0,1	Cloudy.
	3	0	38	48	30,34	66		W	1	Cloudy.
22	8	0	28	43	30,30	70		SW	1	Thick fog.
	3	0	38	49	30,33	64		E	1	Cloudy.
23	8	0	27	42	30,27	68		E	1	Fine, rather hazy.
	3	0	31	49	30,23	61		E	1	Fine.
24	8	0	24	41	30,17	65		E	1	Fine.
	3	0	32	50	30,20	63		E	1	Fine.
25	8	0	23	42	30,22	66		E	1	Fine, rather hazy.
	3	0	32	50	30,19	62		E	1	Fine.
26	8	0	27	42	30,06	67		E	1	Cloudy.
	3	0	33	47	30,02	63		E	1	Fine.
27	8	0	27	41	30,11	72		N	1	Hazy.
	3	0	34	45	30,05	66		E by S	1	Cloudy.
28	8	0	32	41	29,87	71		SE	1	Thick and cloudy.
	3	0	40	45	29,68	73		SW	1	Cloudy.

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for March, 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H.	M.	°	°	Inches.	°		Points.	Str.	
Mar. 1	8	0	40	44	29,13	79	Rain this Month 0,718 Inches.	S by W	2	Cloudy.
	3	0	38	46	28,88	74		NW	1	Rain.
2	8	0	33	43	28,92	73		SW	1	Fine.
	3	0	37	48	28,80	74		SW	1	Cloudy.
3	8	0	34	44	28,85	76		E	1	Cloudy.
	3	0	40	48	28,95	68		S	1	Cloudy.
4	8	0	34	46	29,19	73		N	1	Cloudy.
	3	0	35	47	29,29	73		N	1	Cloudy.
5	8	0	33	44	29,55	74		N	1	Cloudy.
	3	0	34	45	29,69	79		N	1	Cloudy.
6	8	0	30	42	29,85	66		N	1	Cloudy.
	3	0	34	42	29,80	64		NE	1	Cloudy.
7	8	0	31	40	29,69	73		SE	1	Snow. Much snow in the [night.
	3	0	29	42	29,75	68		E	1	Cloudy.
8	8	0	29	40	29,81	73		N	1	Snow.
	3	0	31	44	29,82	66		E	1	Cloudy.
9	8	0	29	40	29,68	74		N	1	Fine.
	3	0	31	43	29,65	73		E	1	Rain.
10	8	0	30	40	29,58	72		E	1	Sleet.
	3	0	35	44	29,52	75		NE	1	Rain.
11	8	0	30	41	29,47	74		N	1	Cloudy.
	3	0	34	44	29,52	71		E	1	Cloudy.
12	8	0	30	41	29,70	73		N	1	Cloudy.
	3	0	35	44	29,75	73		N	1	Snow.
13	8	0	32	41	29,99	74		N	1	Cloudy.
	3	0	34	42	30,07	71		NE	1	Cloudy.
14	8	0	31	39	30,18	70		N	1	Cloudy.
	3	0	33	43	30,21	69		NNE	1	Cloudy.
15	8	0	30	40	30,33	70		E	1	Cloudy.
	3	0	34	43	30,36	67		W	1	Cloudy.
16	8	0	32	40	30,39	72		E	1	Cloudy.
	3	0	36	46	30,39	70		N	1	Cloudy.

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for March, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Mar. 17	8	0	32	40	30,31	72	Rain this Month 0,718 Inches.	E	1	Cloudy.
	3	0	35	44	30,31	71		E	1	Cloudy.
18	8	0	29	42	30,27	75		N	1	Cloudy.
	3	0	32	45	30,19	73		N	1	Cloudy.
19	8	0	29	42	30,05	74		NE	1	Cloudy.
	3	0	31	43	29,93	74		SE	1	Cloudy.
20	8	0	31	40	29,82	74		SSE	1	Cloudy.
	3	0	42	45	29,75	73		SSW	1	Fine.
21	7	30	40	43	29,69	75		E	1	Thick and cloudy.
	3	0	45	46	29,67	77		S	1	Rain.
22	7	30	44	46	29,62	80		SSE	1	Rain.
	3	0	48	49	29,68	68		S	1	Cloudy.
23	7	30	40	47	29,77	73		E	1	Hazy.
	3	0	50	51	29,80	63		SW	1	Cloudy.
24	7	30	38	48	29,80	73		S	1	Cloudy.
	3	0	45	52	29,67	74		S	1	Rain.
25	7	30	42	51	29,66	76		W	1	Cloudy.
	3	0	49	55	29,66	69		WSW	1	Cloudy.
26	7	30	39	51	29,65	73		S	1	Cloudy.
	3	0	50	53	29,66	64		SW	1	Cloudy.
27	7	30	43	51	29,87	73		W	1	Cloudy.
	3	0	52	54	29,91	66		W	1	Fine.
28	7	30	45	50	29,88	73		SSE	1	Fine.
	3	0	52	54	29,76	63		SE	1	Cloudy.
29	7	30	45	52	29,57	67		E	1	Cloudy.
	3	0	49	55	29,58	71		W	1	Cloudy.
30	7	30	41	52	29,78	73		S	1	Foggy.
	3	0	54	58	29,70	60		W	1	Fine.
31	7	30	46	52	29,96	71		S	1	Cloudy.
	3	0	49	56	29,88	73		S	2	Rain.

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1814	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H. M.	°	°	Inches.	°		Points.	Str.	
Apr. 1	7 30	47	54	29,74	76	Rain this Month 1,241 Inches.	S	1	Cloudy.
	3 0	53	60	29,67	63		S	1	Fine.
2	7 30	46	54	29,41	77		S	2	Fine.
	3 0	51	57	29,38	66		SW	2	Cloudy.
3	7 30	46	53	29,40	71		SW	1	Fine.
	3 0	52	59	29,48	56		W	1	Fine.
4	7 30	43	53	29,61	70		NE	1	Cloudy.
	3 0	53	57	29,65	60		E	1	Cloudy.
5	7 30	44	53	29,74	70		NE	1	Fine.
	3 0	52	57	29,78	60		E	1	Cloudy.
6	7 30	46	54	29,96	75		W	1	Thick and hazy.
	3 0	54	59	30,00	62		W	1	Fine.
7	7 30	46	54	30,11	72		W	1	Cloudy.
	3 0	55	59	30,16	63		E	1	Fine.
8	7 30	46	54	30,25	73		E	1	Cloudy and hazy.
	3 0	53	62	30,26	57		E	1	Fine.
9	7 0	44	55	30,24	45		N	1	Fine.
	3 0	55	62	30,20	54		E	1	Fine.
10	7 0	46	55	30,13	46		E	1	Fine.
	3 0	56	62	30,09	54		SE	1	Fine.
11	7 0	46	55	30,04	71		E	1	Fine.
	3 0	57	62	30,00	59		E	1	Fine.
12	7 0	44	55	29,92	70		E	1	Cloudy.
	3 0	62	64	29,87	54		SW	1	Fair.
13	7 0	56	58	29,86	61		N	1	Hazy.
	3 0	64	64	29,81	65		E	1	Fine.
14	7 0	53	60	29,72	63		E	1	Fine, rather hazy.
	3 0	54	67	29,68	62		SW	1	Fair.
15	7 0	54	61	29,59	65		SE	1	Fine.
	3 0	57	63	29,45	65		W	2	Rain.
16	7 0	52	59	29,51	67		S	2	Cloudy.
	3 0	58	62	29,51	63		S	2	Cloudy.

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for April, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Apr. 17	7	0	54	59	29,32	74	Rain this Month 1,241 Inches.	SE	2	Rain.
	3	0	58	62	29,47	60		SW	1,2	Fine.
18	7	0	50	57	29,66	68		S	1	Rain.
	3	0	59	61	29,57	65		W	1	Cloudy.
19	7	0	51	57	29,67	68		SE	1	Fine.
	3	0	57	63	29,69	59		S	1	Fine.
20	7	0	52	58	29,73	69		E	1	Cloudy.
	3	0	55	59	29,73	59		E	1	Cloudy.
21	7	0	51	58	29,73	70		W	1	Fine.
	3	0	55	60	29,71	60		W	1	Cloudy.
22	7	0	45	56	29,83	66		W	1	Cloudy.
	3	0	54	58	29,84	55		NW	1	Cloudy.
23	7	0	43	54	30,06	66		W	1	Cloudy.
	3	0	51	56	30,02	65		S	1	Rain.
24	7	0	45	54	29,83	67		NW	1	Cloudy.
	3	0	49	55	29,83	59		NE	1	Cloudy.
25	7	0	44	52	29,86	68		N	1	Cloudy.
	3	0	49	54	29,88	67		N	1	Cloudy.
26	7	0	44	51	29,91	69		N	1	Cloudy.
	3	0	50	56	29,99	63		N	1	Cloudy.
27	7	0	43	52	30,15	69		N	1	Fine.
	3	0	48	54	30,16	63		E	1	Cloudy.
28	7	0	44	52	30,17	67		SE	1	Cloudy.
	3	0	49	56	30,17	67		S	1,2	Rain.
29	7	0	45	54	30,11	70		SSE	1	Rain.
	3	0	53	55	30,07	62		S	1	Cloudy.
30	7	0	50	53	30,06	62		S	2	Cloudy.
	3	0	58	59	30,10	54		S	1,2	Cloudy.

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for May, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.		Winds. Points.	Winds. Str.	Weather.
May 1	7	0	51	54	30,19	° 57	Rain this Month 1,779 Inches.	S	1	Fair.
	3	0	60	61	30,20	54		SW	1	Fine.
2	7	0	54	56	30,20	59		W	1	Cloudy.
	3	0	57	58	30,19	69		E	1	Cloudy.
3	7	0	51	54	30,08	63		E	1	Fine.
	3	0	56	63	29,97	59		SE	1	Fair.
4	7	0	47	56	29,91	62		NE	1	Cloudy.
	3	0	50	58	29,82	58		E	1	Cloudy.
5	7	0	43	53	29,60	63		E	1	Cloudy.
	3	0	52	54	29,41	68		E	1	Rain.
6	7	0	48	54	29,43	77		SE	1	Cloudy.
	3	0	55	56	29,44	76		W	1	Rain.
7	7	0	48	55	29,76	76		E	1	Cloudy.
	3	0	59	57	29,88	63		W	1	Cloudy.
8	7	0	53	56	30,01	70		W	1	Cloudy.
	3	0	62	61	30,04	58		N	1	Cloudy.
9	7	0	50	55	30,19	59		E	1	Cloudy.
	3	0	52	57	30,23	60		E	1	Cloudy.
10	7	0	44	54	30,33	64		NE	1	Cloudy.
	3	0	52	59	30,36	56		NE	1	Cloudy.
11	7	0	44	53	30,46	62		E	1	Cloudy.
	3	0	52	59	30,43	56		E	1	Fair.
12	7	0	44	53	30,39	65		N	1	Cloudy.
	3	0	53	60	30,34	55		E	1	Cloudy.
13	7	0	48	56	30,18	70		NW	1	Cloudy.
	3	0	48	57	30,06	71		N	1	Cloudy.
14	7	0	45	55	30,02	72		N	1	Cloudy.
	3	0	50	56	30,03	65		N	1	Cloudy.
15	7	0	47	54	30,03	67		N	1	Cloudy.
	3	0	53	57	29,98	61		N	1	Cloudy.
16	7	0	44	54	29,98	67		SE	1	Cloudy.
	3	0	54	56	29,97	58		NE	1	Cloudy.

METEOROLOGICAL JOURNAL

for May, 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
May 17	7	0	47	55	30,12	71	Rain this Month 1,779 Inches.	E	1	Cloudy.
	3	0	58	62	30,14	55		E	1	Cloudy.
18	7	0	51	56	30,17	67		N	1	Cloudy.
	3	0	60	63	30,16	53		E	1	Fair.
19	7	0	52	55	30,11	56		NE	1	Fair.
	3	0	62	64	30,09	55		E	1	Fine.
20	7	0	51	59	30,03	65		N	1	Cloudy.
	3	0	62	66	29,99	51		SE	1	Fair.
21	7	0	48	59	29,92	63		NNE	1	Fair.
	3	0	58	64	29,84	59		SE	1	Fine.
22	7	0	50	57	29,78	60		N	1	Cloudy.
	3	0	52	58	29,75	54		N	1	Cloudy.
23	7	0	42	55	29,67	64		N	1	Cloudy.
	3	0	53	58	29,59	54		N W	1	Cloudy.
24	7	0	43	54	29,57	70		W	1	Rain.
	3	0	46	56	29,56	73		N	1	Rain.
25	7	0	44	54	29,63	74		N	1	Cloudy.
	3	0	46	56	29,75	63		N	1	Cloudy.
26	7	0	46	54	29,93	60		W	1	Fine.
	3	0	51	57	29,95	54		SW	1	Cloudy.
27	7	0	47	55	29,92	66		E	1	Cloudy.
	3	0	56	59	29,85	56		SE	1	Fair.
28	7	0	49	56	29,74	70		N	1	Fine.
	3	0	62	62	29,66	55		S	1	Fine.
29	7	0	58	58	29,73	61		N	1	Fine.
	3	0	63	61	29,78	53		N	1	Fine.
30	7	0	52	58	30,04	63		W	1	Fine.
	3	0	61	61	30,09	54		NE	1	Cloudy.
31	7	0	54	59	30,09	65		S	1	Cloudy.
	3	0	62	61	30,06	53		W	1	Cloudy.

METEOROLOGICAL JOURNAL

for June, 1814.

1814	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Str.	Weather.
June 1	7	0	50	58	30,05	67	Rain this Month 1,558 Inches.	E	1	Cloudy and hazy.
	3	0	55	59	29,99	64		E	1	Cloudy.
2	7	0	52	58	29,86	75		E	1	Rain.
	3	0	56	58	29,85	75		NNE	1	Cloudy.
3	7	0	52	58	29,84	73		E	1	Cloudy.
	3	0	54	58	29,76	78		E	1	Rain.
4	7	0	51	57	29,75	76		E	1	Rain.
	3	0	52	57	29,80	72		NE	1	Cloudy.
5	7	0	50	56	29,98	71		N	1	Cloudy.
	3	0	51	56	30,09	68		N	1	Cloudy.
6	7	0	49	55	30,13	65		E	1	Cloudy.
	3	0	56	55	30,13	62		E	1	Cloudy.
7	7	0	47	54	30,07	65		NE	1	Cloudy.
	3	0	51	55	30,05	60		E	1	Cloudy.
8	7	0	47	54	29,98	67		N	1	Cloudy.
	3	0	59	59	29,98	51		NE	1	Cloudy.
9	7	0	49	55	30,04	70		N	1	Fine.
	3	0	58	62	30,09	54		E	1	Fair.
10	7	0	50	57	30,11	68		E	1	Cloudy.
	3	0	59	62	30,12	56		E	1	Fine.
11	7	0	53	58	30,05	68		E	1	Cloudy.
	3	0	61	63	30,01	57		E	1	Cloudy.
12	7	0	54	58	29,80	72		E	1	Cloudy.
	3	0	65	63	29,84	61		NW	1	Cloudy.
13	7	0	54	59	30,02	70		E	1	Cloudy.
	3	0	64	61	30,05	64		E	1	Cloudy.
14	7	0	64	61	30,11	71		W	1	Cloudy.
	3	0	73	65	30,09	58		W	1	Fine.
15	7	0	64	64	29,92	79		S	1	Cloudy. A thunder storm in [the night.
	3	0	68	68	29,88	67		S	1	Fine.
16	7	0	59	62	29,99	66		W	1	Cloudy.
	3	0	64	65	29,96	66		SW	1	Cloudy.

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for June, 1814.

1814	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Winds. Points.	Str.	Weather.
June 17	7	0	54	61	30,10	66°	NW	1	Fine.
	3	0	62	62	30,11	59	NW	1	Cloudy.
18	7	0	56	61	30,13	64	W	1	Cloudy.
	3	0	62	61	30,00	66	W	1	Cloudy.
19	7	0	57	61	29,92	76	W	1	Rain.
	3	0	63	62	29,82	63	NNW	1	Cloudy.
20	7	0	52	59	29,83	62	N	1	Cloudy.
	3	0	59	59	29,79	63	N	1	Cloudy.
21	7	0	53	58	29,74	63	N	1	Cloudy.
	3	0	59	60	29,80	57	N	1	Cloudy.
22	7	0	53	58	29,90	70	N	1	Cloudy.
	3	0	55	59	29,97	70	E	1	Rain.
23	7	0	52	57	30,10	71	N	1	Cloudy.
	3	0	57	59	30,17	65	NNE	1	Cloudy.
24	7	0	52	57	30,25	68	N	1	Cloudy.
	3	0	57	58	30,32	65	N	1	Cloudy.
25	7	0	53	57	30,33	64	N	1	Cloudy.
	3	0	57	58	30,28	64	N	1	Cloudy.
26	7	0	53	57	30,12	65	N	1	Cloudy.
	3	0	58	58	30,08	63	NNE	1	Cloudy.
27	7	0	53	57	30,08	68	N	1	Cloudy.
	3	0	58	58	30,08	62	N	1	Cloudy.
28	7	0	56	57	30,02	69	NW	1	Cloudy.
	3	0	63	60	29,97	61	N	1	Cloudy.
29	7	0	54	58	29,91	71	SSE	1	Hazy.
	3	0	67	60	29,90	59	NW	1	Cloudy.
30	7	0	60	60	29,91	67	N	1	Cloudy.
	3	0	64	66	29,94	56	N	1	Fine.

Rain this Month 1,558 Inches.

METEOROLOGICAL JOURNAL

for July 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
July 1	7	0	59	61	29,91	62	Rain this Month 0,676 Inches.	W	1	Cloudy.
	3	0	63	62	29,89	59		N	1	Cloudy.
2	7	0	56	60	29,98	61		N	1	Fine.
	3	0	64	66	30,02	55		W	1	Fair.
3	7	0	60	61	30,03	61		SW	1	Fine.
	3	0	68	65	30,02	55		NW	1	Cloudy.
4	7	0	59	62	30,05	65		W	1	Fine.
	3	0	67	64	30,05	56		W	1	Cloudy.
5	7	0	61	63	30,03	75		W	1	Cloudy.
	3	0	72	68	30,04	56		NW	1	Cloudy.
6	7	0	58	63	30,07	67		W	1	Cloudy.
	3	0	70	71	30,05	57		W	1	Fine.
7	7	0	58	63	29,97	66		S	1	Fair.
	3	0	71	71	29,90	58		W	1	Cloudy.
8	7	0	60	66	29,92	68		W	1	Cloudy.
	3	0	65	66	29,87	69		SW	1,2	Rain.
9	7	0	61	65	29,78	73		SW	1	Cloudy.
	3	0	64	66	29,77	68		SW	1	Cloudy.
10	7	0	61	65	29,75	71		S	1	Cloudy.
	3	0	64	64	29,75	69		W	1	Cloudy.
11	7	0	58	62	29,91	68		SW	1	Fine.
	3	0	65	65	29,95	62		W	1	Fine.
12	7	0	57	63	30,10	65		W	1	Fine.
	3	0	66	65	30,12	57		NNE	1	Cloudy.
13	7	0	60	64	30,14	64		W	1	Cloudy.
	3	0	62	64	29,99	58		W	1	Cloudy.
14	7	0	55	62	29,87	65		W	1	Fine.
	3	0	63	64	29,82	59		W	1	Cloudy.
15	7	0	57	62	29,79	65		NW	1	Cloudy.
	3	0	63	63	29,76	63		W	1	Cloudy and hazy.
16	7	0	58	62	29,70	70		N	1	Cloudy.
	3	0	63	63	29,79	65		E	1	Cloudy.

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for July 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain	Winds. Points.	Winds. Str.	Weather.
July 17	7	0	54	61	29,94	70	Rain this Month 0,676 Inches.	E	1	Cloudy.
	3	0	64	65	29,96	62		SW	1	Cloudy.
18	7	0	57	62	29,95	70		W	1	Cloudy.
	3	0	67	65	29,92	64		S	1	Cloudy.
19	7	0	58	62	29,93	68		W	1	Cloudy.
	3	0	67	65	29,88	60		W	1,2	Cloudy.
20	7	0	61	63	29,73	69		S	2	Cloudy.
	3	0	67	65	29,67	61		S	2	Cloudy.
21	7	0	59	63	29,77	69		W	1	Fair.
	3	0	69	67	29,85	56		E	1	Cloudy.
22	7	0	61	64	29,95	65		E	1	Fine.
	3	0	67	66	30,27	56				
23	7	0	58	64	30,27	68		W	1	Fair.
	3	0	71	71	30,27	60		S	1	Fine.
24	7	0	56	66	30,23	65		SE	1	Fair.
	3	0	72	76	30,11	60		E	1	Fair.
25	7	0	65	67	29,94	67		E	1	Fine.
	3	0	74	75	29,85	56		SSE	1	Fine.
26	7	0	64	69	29,87	67		W	1	Fine.
	3	0	73	76	29,90	58		W	1	Cloudy.
27	7	0	65	70	30,04	67		ENE	1	Fine.
	3	0	76	77	30,03	59		E	1	Fine.
28	7	0	70	71	29,94	70		E	1	Fair.
	3	0	79	81	29,89	55		SE	1	Cloudy.
29	7	0	66	74	29,80	67		W	1	Cloudy.
	3	0	71	75	29,86	57		W	1,2	Cloudy.
30	7	0	61	69	30,01	68		W	1	Fine.
	3	0	71	74	30,07	56		W	1,2	Fine.
31	7	0	62	69	30,11	64		W	1	Fine.
	3	0	73	73	30,04	56		W	1	Fine.

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for August, 1814.

1814	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Str.	Weather.
Aug. 1	7	0	63	69	29,92	64°	Rain this Month 2,000 Inches.	NW	1	Cloudy.
	3	0	72	74	29,91	57		NW	1	Fine.
2	7	0	62	67	30,08	66		W	1	Fine.
	3	0	71	73	30,11	60		SW	1	Fine.
3	7	0	60	69	30,06	68		W	1	Fair.
	3	0	72	73	29,96	57		SW	1	Cloudy.
4	7	0	56	67	30,12	66		W	1	Fair.
	3	0	69	71	30,12	57		WSW	1	Fair.
5	7	0	60	66	29,88	66		S	1	Fine.
	3	0	68	70	29,79	61		SW	1	Fine.
6	7	0	59	66	29,75	66		W	1	Cloudy.
	3	0	68	70	29,87	58		NNW	1	Fine.
7	7	0	58	66	30,03	66		W	1	Cloudy.
	3	0	67	68	29,97	59		W	1,2	Cloudy.
8	7	0	58	65	29,73	70		N	1	Cloudy.
	3	0	66	67	29,71	60		W	2	Cloudy.
9	7	0	56	64	29,92	64		W	1	Fine.
	3	0	65	68	30,02	55		NW	1	Cloudy.
10	7	0	58	64	30,05	68		W by N	1	Cloudy.
	3	0	65	66	30,05	60		NW	1	Cloudy.
11	7	0	60	64	30,18	66		NW	1	Cloudy.
	3	0	67	65	30,15	57		W	1	Cloudy.
12	7	0	57	64	30,14	70		W	1	Cloudy.
	3	0	68	66	30,11	64		W	1	Cloudy.
13	7	0	59	64	29,99	68		SW	1,2	Cloudy.
	3	0	66	66	29,89	63		SW	1,2	Cloudy.
14	7	0	55	63	29,88	66		N N W	1	Fair.
	3	0	65	68	29,86	52		W	1	Fine.
15	7	0	53	62	29,91	65		W	1	Fair.
	3	0	65	65	29,92	56		W	1	Cloudy.
16	7	0	59	63	29,87	71		S	1	Rain.
	3	0	61	64	29,81	59		NW	1	Cloudy.

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for August, 1814.

1814	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Str.	Weather.
Aug. 17	7	0	54	61	29,93	68°	Rain this Month 2,000 Inches.	W	1	Fair.
	3	0	65	64	29,99	56		WNW	1	Fine.
18	7	0	62	63	30,02	69		W	1	Cloudy.
	3	0	68	66	30,05	57		W	1	Cloudy.
19	7	0	59	64	30,06	60		W	1	Cloudy and hazy.
	3	0	63	63	29,99	56		NNW	1	Cloudy.
20	7	0	52	61	30,00	66		W	1	Fine.
	3	0	60	63	30,03	62			1	Cloudy.
21	7	0	56	62	29,98	66		S	1	Cloudy.
	3	0	63	63	29,89	68		S	2	Cloudy.
22	7	0	58	63	29,87	69		W	1	Cloudy.
	3	0	66	68	29,89	59		SW	1	Fine.
23	7	0	63	64	29,74	73		E	1	Fine.
	3	0	70	66	29,67	58		S	1	Cloudy.
24	7	0	58	64	29,67	69		E	1	Cloudy.
	3	0	60	64	29,55	77		NE	1	Rain.
25	7	0	58	63	29,64	77		S	1	Cloudy.
	3	0	62	65	29,66	65		NNW	1	Rain.
26	7	0	58	61	29,79	68		W	1	Fine.
	3	0	63	67	29,84	57		NNW	1	Fine.
27	7	0	51	62	29,94	68		W	1	Hazy.
	3	0	61	66	29,97	56		N	1	Fine.
28	7	0	51	61	29,99	68		NNW	1	Fair.
	3	0	61	66	30,05	55		E	1	Fine.
29	7	0	52	60	30,07	64		N	1	Fair.
	3	0	65	68	30,17	54		W	1	Fair.
30	7	0	57	63	30,22	69		W	1	Cloudy.
	3	0	65	68	30,21	59		WNW	1	Cloudy.
31	7	0	61	65	30,29	70		W	1	Cloudy.
	3	0	69	67	30,30	64		N	1	Cloudy.

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for September, 1814.

1814	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Str.	Weather.
Sep. 1	7 30	60	64	30,36	69°	Rain this Month 0,965 Inches.	E	1	Fine.
	3 0	65	67	30,37	59		E	1	Fine.
2	7 30	58	63	30,32	66		NW	1	Fine.
	3 0	63	70	30,27	60		E	1	Fair.
3	7 30	57	64	30,24	68		N	1	Cloudy and thick.
	3 0	63	65	30,23	60		N	1	Cloudy.
4	7 30	52	61	30,31	64		N	1	Fair.
	3 0	61	64	30,26	58		E	1	Cloudy.
5	7 30	52	61	30,23	64		N	1	Fair.
	3 0	61	64	30,21	55		NW	1	Cloudy.
6	7 30	54	62	30,18	66		W	1	Cloudy.
	3 0	64	65	30,07	57		W	1	Cloudy.
7	7 30	53	61	29,81	68		W	1	Cloudy.
	3 0	55	62	29,78	65		NNE	1	Rain.
8	7 30	56	61	29,99	72		NE	1	Cloudy.
	3 0	60	62	30,10	61		N	1	Cloudy.
9	7 30	54	60	30,14	70		N	1	Cloudy.
	3 0	61	65	30,15	59		N	1	Fine.
10	7 30	54	61	30,15	65		N	1	Fine.
	3 0	58	62	30,15	58		NE	1	Cloudy.
11	7 30	48	59	30,15	66		N	1	Fair.
	3 0	60	65	30,17	57		N	1	Fine.
12	7 30	49	59	30,25	65		E	1	Cloudy.
	3 0	57	61	30,23	58		N	1	Cloudy.
13	7 30	44	58	30,23	66		SW	1	Fine, rather hazy.
	3 0	57	61	30,20	59		N	1	Cloudy.
14	7 30	44	57	30,21	68		W	1	Thick and hazy.
	3 0	58	61	30,22	59		E	1	Cloudy.
15	7 30	49	58	30,22	68		NE	1	Hazy.
	3 0	60	65	30,17	61		E	1	Fine.
16	7 30	50	59	30,13	71		N	1	Fine, rather hazy.
	3 0	60	60	30,10	71		N	1	Hazy.

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for September, 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Sep. 17	7	30	52	60	30,09	71°	Rain this Month 0,965 Inches.	NE	1	Hazy.
	3	0	62	66	30,08	55		S	1	Fair.
18	7	30	56	61	30,12	66		E	1	Hazy.
	3	0	62	67	30,16	67		NE	1	Hazy.
19	7	30	53	58	30,16	68		NNE	1	Cloudy and hazy.
	3	0	64	72	30,13	55		S	1	Fair.
20	7	30	57	64	30,08	63		S	1	Fine.
	3	0	67	71	30,02	56		S	1	Fine.
21	7	30	59	66	29,89	71		S	2	Cloudy.
	3	0	63	66	29,83	62		S	1,2	Cloudy.
22	7	30	54	63	29,83	69		W	1	Cloudy.
	3	0	61	65	29,82	63		W	1	Cloudy.
23	7	30	56	62	29,98	70		S	1	Fine.
	3	0	58	63	29,90	71		S	1	Rain.
24	7	30	63	65	29,80	72		S	2	Rain.
	3	0	65	65	29,65	78		S	1	Fine.
25	7	30	62	64	29,70	70		SW	1	Rain.
	3	0	63	64	29,55	77		SW	1	Fine.
26	7	30	61	63	29,80	63		SE	1	Fine.
	3	0	60	62	29,85	68		SE	1	Fine.
27	7	30	58	62	29,80	66		W	1	Rain.
	3	0	59	63	29,75	72		W	1	Cloudy
28	7	30	55	61	29,85	62		N	1	Fine.
	3	0	58	62	29,85	70		N	1	Fine.
29	7	30	57	62	29,95	64		N	1	Fine.
	3	0	55	61	30,00	69		N	1	Fine.
30	7	30	59	60	30,00	70		NE	1	Fine.
	3	0	58	61	30,05	68		NE	1	Fine.

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for October, 1814.

1814	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.		Winds. Points.	Str.	Weather.
Oct. 1	7 30	54	60	30,05	° 68	Rain this Month 1,611 Inches.	NE	2	Fine.
	3 0	55	59	30,10	62		NE	1	Fine.
2	7 30	52	60	30,15	61		NE	2	Fine.
	3 0	60	57	30,15	60		NE	1	Fine.
3	7 30	56	61	30,10	60		NE	1	Fine.
	3 0	52	56	30,15	76		NE	1	Fine.
4	7 30	58	58	30,20	66		NE	1	Fine.
	3 0	54	57	30,15	67		NE	1	Fine.
5	7 30	46	54	30,13	70		NNE	1	Hazy.
	3 0	55	61	30,05	60		SE	1	Fair.
6	7 30	40	53	29,88	70		W	1	Cloudy and hazy.
	3 0	55	57	29,78	62		W	1,2	Cloudy.
7	7 30	43	54	29,80	70		W	1	Fine, rather hazy.
	3 0	51	57	29,84	60		N	1	Cloudy.
8	7 30	42	52	29,90	65		NW	1	Fine.
	3 0	51	56	29,94	61		W	1	Fine, rather hazy.
9	7 30	41	51	30,12	64		NNE	1	Fine.
	3 0	48	55	30,13	60		N	1	Cloudy.
10	7 30	34	50	30,22	65		W	1	Fine, rather hazy.
	3 0	45	53	30,19	57		SW	1	Fine.
11	7 30	46	51	29,99	65		S	1	Cloudy.
	3 0	52	52	29,89	63		W	1	Cloudy.
12	7 30	48	51	29,75	66		SSE	1	Cloudy.
	3 0	54	53	29,71	65		SSE	1	Cloudy.
13	7 30	55	53	29,63	69		S	1	Fine.
	3 0	58	58	29,61	66		SE	1	Cloudy.
14	7 30	55	55	29,48	73		SSE	1	Cloudy.
	3 0	60	59	29,42	62		SE	1,2	Cloudy.
15	7 30	50	57	29,54	72		W	1	Fine.
	3 0	58	59	29,54	70		W	1	Fine.
16	7 30	46	55	29,57	69		W	1	Fine.
	3 0	54	59	29,73	59		W	1	Cloudy.

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for October, 1814.

1814	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Oct. 17	7 30	47	55	29,77	69	Rain this Month 1,611 Inches.	W	1	Cloudy.
	3 0	55	58	29,70	66		SW	1	Cloudy.
18	7 30	53	56	29,40	72		S	2	Cloudy.
	3 0	52	58	29,26	73		W	1	Rain.
19	7 30	46	55	29,22	70		E	1,2	Cloudy.
	3 0	52	58	29,20	69		S	2	Rain.
20	7 30	43	55	29,27	72		NW	1	Fine.
	3 0	52	57	29,43	63		W	1	Cloudy.
21	7 30	39	53	29,74	71		W	1	Fine.
	3 0	52	58	29,88	66		W	1	Cloudy.
22	7 30	52	55	29,80	76		W	1	Cloudy.
	3 0	55	60	29,78	65		W	1	Cloudy.
23	7 30	50	56	29,62	66		NW	1	Fine.
	3 0	50	57	29,62	61		W	1	Fine.
24	7 30	41	53	29,83	69		W	1	Fine.
	3 0	49	58	29,83	61		W	1	Cloudy. [night.
25	7 30	46	55	29,13	74		W	1	Rain. Wind and rain in the
	3 0	50	56	29,15	64		W	1	Cloudy.
26	7 30	41	52	29,54	72		NE	1	Cloudy.
	3 0	48	56	29,66	70		N	1	Cloudy.
27	7 30	44	53	29,80	74		N	1	Cloudy.
	3 0	49	54	29,82	70		W	1	Cloudy.
28	7 30	42	52	29,88	72		SW	1	Fine, rather hazy.
	3 0	51	54	29,88	65		S	1	Cloudy.
29	7 30	45	52	29,82	74		E	1	Foggy.
	3 0	50	52	29,73	75		E	1	Rain.
30	7 30	43	52	29,90	73		W	1	Hazy.
	3 0	48	53	29,93	70		E	1	Cloudy.
31	7 30	46	52	29,95	75		N	1	Cloudy.
	3 0	53	53	29,94	70		E	1,2	Cloudy.

METEOROLOGICAL JOURNAL

for November, 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H.	M.	°	°	Inches.	°		Points.	Str.	
Nov. 1	8	0	48	52	29,99	73	Rain this Month 1,864 Inches.	N	1	Thick and hazy.
	3	0	48	52	29,98	72		N	1	Cloudy.
2	8	0	44	52	30,00	70		N	1	Cloudy.
	3	0	43	52	30,02	68		N	1	Cloudy.
3	8	0	40	50	29,99	69		N	1	Cloudy.
	3	0	46	52	29,84	66		N	1	Cloudy.
4	8	0	38	49	29,89	74		N	1	Foggy.
	3	0	43	50	29,88	70		N	1	Cloudy.
5	8	0	41	49	29,85	73		N	1	Cloudy.
	3	0	44	51	29,78	75		N	1	Rain.
6	8	0	36	49	29,85	75		W	1	Thick fog.
	3	0	43	49	29,88	71		N	1	Fair.
7	8	0	40	47	29,81	73		W	1	Fine.
	3	0	48	49	29,65	69		W	1	Cloudy.
8	8	0	37	48	29,37	72		W	1	Fine.
	3	0	40	48	29,32	70		W	1	Fine.
9	8	0	36	47	29,44	69		W	1	Fine, rather hazy.
	3	0	42	48	29,47	66		W	1	Rain.
10	8	0	33	44	29,98	65		NNW	1	Hazy.
	3	0	40	51	30,18	63		NW	1	Fine.
11	8	0	34	46	30,31	71		W	1	Hazy.
	3	0	43	50	30,25	65		W	1	Cloudy.
12	8	0	48	52	29,85	74		W	1	Rain.
	3	0	49	53	29,90	62		NW	1	Fine.
13	8	0	44	50	29,89	72		W	1	Fine.
	3	0	46	51	29,89	65		NW	1	Cloudy.
14	8	0	45	50	29,87	76		W	1	Cloudy.
	3	0	52	55	29,81	74		W	1	Cloudy.
15	8	0	48	54	29,91	75		SW	1	Cloudy.
	3	0	50	56	29,87	72		NW	1	Cloudy.
16	8	0	47	52	29,35	74		S	2,3	Rain.
	3	0	48	55	29,69	65		NW	2	Fine.

METEOROLOGICAL JOURNAL
for November 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H.	M.	°	°	Inches.	°		Points.	Str.	
Nov. 17	8	0	43	51	30,06	73	Rain this Month 1,864 Inches.	W	1	Fine.
	3	0	53	56	29,95	75		SW	1	Fine.
18	8	0	48	54	29,91	73		SW	1	Cloudy.
	3	0	50	57	29,73	67		S	2	Cloudy.
19	8	0	45	54	29,43	73		W	1	Cloudy.
	3	0	46	58	29,49	66		NW	1	Cloudy.
20	8	0	39	53	29,51	71		NW	1	Cloudy.
	3	0	40	53	29,60	74		NE	1	Cloudy.
21	8	0	37	51	29,62	71		NE	1	Fine.
	3	0	39	52	29,66	64		N	1	Fair.
22	8	0	28	46	29,71	69		W	1	Hazy.
	3	0	39	52	29,73	68		NW	1	Fair.
23	8	0	30	47	29,69	71		N	1	Foggy.
	3	0	38	51	29,70	72		E	1	Fine.
24	8	0	31	46	29,83	74		E	1	Thick and foggy.
	3	0	41	50	29,80	73		S	1	Cloudy.
25	8	0	46	49	29,55	77		S	1	Rain.
	3	0	50	53	29,48	78		S	1	Rain.
26	8	0	46	53	29,27	75		W	1	Cloudy.
	3	0	46	53	29,46	73		W	1	Fine.
27	8	0	39	49	29,64	71		W	1	Fine.
	3	0	44	50	29,54	69		W	1	Rain.
28	8	0	38	48	29,69	77		W	1	Hazy.
	3	0	45	51	29,56	76		SW	1	
29	8	0	44	49	29,48	75		W	1	Cloudy.
	3	0	46	54	29,46	74		W	1	Fine.
30	8	0	42	48	29,32	73		NW	1	Fine.
	3	0	43	53	29,17	71		N	1	Fine.

METEOROLOGICAL JOURNAL

for December, 1814.

1814	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Dec. 1	8	0	38	49	29,48	73	Rain this Month 2,250 Inches.	NW	1	Cloudy.
	3	0	43	55	29,64	69		NW	1	Cloudy.
2	8	0	36	49	29,78	72		NW	1	Foggy.
	3	0	40	52	29,80	70		NW	1	Thick and hazy.
3	8	0	36	50	29,92	73		SE	1	Cloudy and foggy.
	3	0	35	48	29,88	75		E	1	Cloudy and foggy.
4	8	0	35	46	29,53	75		S	1	Cloudy.
	3	0	45	52	29,46	75		SW	1	Cloudy.
5	8	0	35	46	29,36	76		W	1	Hazy.
	3	0	46	52	29,70	73		N	1	Cloudy.
6	8	0	34	47	29,98	72		NE	1	Fine.
	3	0	39	50	30,07	70		NNW	1	Fine.
7	8	0	41	48	29,87	69		S	2	Rain.
	3	0	42	54	29,74	75		SW	1	Cloudy.
8	8	0	46	48	29,66	78		W	1,2	Cloudy.
	3	0	48	52	29,46	75		SW	2	Rain.
9	8	0	49	52	29,31	75		W	1	Cloudy.
	3	0	45	55	29,47	67		NW	1,2	Cloudy.
10	8	0	33	49	29,76	70		E	1	Cloudy.
	3	0	40	52	29,46	76		E	1,2	Rain.
11	8	0	49	52	29,44	75		W	1	Rain.
	3	0	51	53	29,58	78		W	1	Cloudy.
12	8	0	53	53	29,70	79		WSW	2	Cloudy.
	3	0	55	57	29,71	78		W	2	Cloudy.
13	8	0	53	56	29,58	73		W	2	Cloudy.
	3	0	53	58	29,55	72		W	2	Cloudy.
14	8	0	45	55	29,81	73		W	1	Fine.
	3	0	52	58	29,72	73		SW	2	Cloudy.
15	8	0	52	55	29,63	73		SW	1,2	Fine.
	3	0	53	58	29,66	69		NW	1	Fine.
16	8	0	54	58	29,47	77		S	2,3	Cloudy.
	3	0	50	58	29,56	65		NW	2	Cloudy.

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for December, 1814.

1814	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.				Weather.
	H.	M.	°	°	Inches.	°	Rain this Month 0,861 Inches.	Points.	Str.	
Dec. 17	8	0	51	57	29,70	70		NW	2	Cloudy.
	3	0	52	57	29,84	75		W	2	Cloudy.
18	8	0	54	57	29,85	72		W	3	Cloudy.
	3	0	55	56	29,89	70		S	1,2	Cloudy.
19	8	0	53	56	29,73	73		W	2	Cloudy.
	3	0	53	58	29,73	71		W	1	Cloudy.
20	8	0	38	52	30,10	70		W	1	Fine rather hazy.
	3	0	42	56	30,11	69		NW	1,2	Fine.
21	8	0	38	48	30,03	68		E	1	Fine.
	3	0	39	53	29,92	67		E	1	Cloudy.
22	8	0	34	48	29,76	65		W	1	Fine.
	3	0	38	51	29,65	66		E	1,2	Cloudy.
23	8	0	35	47	29,48	68		E	1	Cloudy.
	3	0	38	50	29,61	66		N	1	Cloudy.
24	8	0	32	41	29,68	66		E	1	Cloudy.
	3	0	32	47	29,65	68		E	1	Cloudy.
25	8	0	31	44	29,62	71		SE	1	Cloudy.
	3	0	32	44	29,63	72		NW	1	Snow.
26	8	0	33	40	29,62	75		W	1	Rain much snow in the night.
	3	0	36	46	29,62	73		E	1	Cloudy.
27	8	0	34	43	29,48	76		E	1	Rain.
	3	0	38	47	29,24	78		S	1	Cloudy.
28	8	0	40	46	29,92	80		E	1	Rain.
	3	0	39	49	29,20	80		N	1,2	Cloudy.
29	8	0	36	45	29,71	72		W	1	Cloudy.
	3	0	38	49	29,76	73		E	1	Cloudy.
30	8	0	43	48	29,86	80		SW	1	Cloudy.
	3	0	47	50	29,81	80		S	1	Rain.
31	8	0	37	48	29,94	77		W	1	Fine.
	3	0	44	52	30,04	75		W	1	Fair.

1814.	Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	46	17	28,8	47	29	38,5	30,20	28,32	29,53	80	62	73,2	1,122
February	48	23	35,6	55	41	46,7	30,46	29,38	30,06	78	61	69,6	0,528
March	54	29	37,5	58	39	45,9	30,39	28,80	29,73	79	63	71,5	0,718
April	64	43	50,8	67	51	57,3	30,26	29,32	29,84	77	45	64,1	1,296
May	63	43	51,8	66	53	57,3	30,46	29,41	29,96	77	51	62,2	1,779
June	73	47	56,5	68	54	59,1	30,33	29,74	30,00	79	51	65,9	1,558
July	79	54	64,0	81	60	66,4	30,33	29,67	29,80	75	55	63,3	0,676
August	72	51	61,6	76	60	65,5	30,30	29,55	29,96	77	52	63,4	2,000
September	65	44	57,5	72	57	62,7	30,37	29,55	30,05	78	55	65,1	0,965
October	60	34	49,5	61	50	55,3	30,22	29,13	29,78	76	57	65,6	1,611
November	53	28	42,7	58	44	50,9	30,31	29,17	29,73	78	62	71,1	1,864
December	55	32	42,6	58	40	51,0	30,11	29,20	29,65	80	65	72,7	2,250
Whole year			48,2			54,7			29,84			67,3	16,367

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

By another Rain Gage placed at a few feet distant from the former and 11 feet 6 inches lower, the quantity of rain this year appears to have been 20,723 inches.

Mean Variation of the Magnetic Needle,	June, 1814,	24°	16′	42″ West.
	July,	24	17	54
	August	24	21	10
	September	24	20	33

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXV.

PART II.

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PHILOSOPHICAL

TRANSACTIONS.

X. *On some phenomena of colours, exhibited by thin plates. By* John Knox, *Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.*

Read April 6, 1815.

It is not without reason that the phenomena of light have been subjects of speculative enquiry from the earliest ages of philosophy; since perhaps no study can be more interesting to the inquisitive mind, than the contemplation of that medium through which it receives its most exalted enjoyments.

There are probably no appearances in physical optics that have excited more attention, and that have been less satisfactorily accounted for, than those prismatic concentric rings which appear between lenses, or between a flat glass and a lens, when laid together. And, notwithstanding some of the most eminent philosophers and opticians have given explanations thereof, particularly Sir Isaac Newton and Dr. Herschel, it will appear from what follows, that the subject is far from being exhausted.

The insufficiency of Newton's theory for the solution of this problem, by the supposed fits of easy transmission and

reflection of the rays of light, is now generally admitted; nor does it appear that any other more satisfactory, has yet been adopted in its stead.

This will not be thought surprising, if it shall appear by the following experiments, that neither NEWTON, nor any other writer that has followed him in the same line, has been in possession of all the phenomena connected with this curious and intricate subject.

I was induced to make the following experiments in consequence of having lately read Dr. HERSCHEL's excellent paper on the same subject, published in the 95th vol. of the Philosophical Transactions, in which he has related a great number of experiments, explaining a variety of phenomena relative to prismatic rings; and must acknowledge my obligations to this celebrated author, for his simple but ingenious contrivance of viewing prismatic phenomena by means of the shadow of a black card; without which, it is probable the following discoveries, such as they are, would not have been made.

Exp. 1. On repeating some of the experiments mentioned by Dr. HERSCHEL in vol. 95 of the Philosophical Transactions, and having by means of the shadow of a blackened card perceived no less than eight sets of rings in some cases, and being very attentive in endeavouring to distinguish transmitted from reflected sets, I faintly saw parallel lines or streaks, which at first were mistaken for the threads of a piece of black silk which was placed under the lower glass, in order to see the rings more distinctly. It was not until after repeated examinations by the light of a lamp, in which the direct light is shaded from the eyes, that it was ascertained,

that those parallel lines were drawn through the intersections made by the several rings of the primary set and its reflected image, and that they consisted of light, although their dimensions were too small for their colours to be distinctly perceived.

The apparatus used in this experiment was a piece of good looking-glass plate laid on the plane side of a plano-concave lens belonging to a compound microscope, and was used for holding mites or other animalcules on the concave side. The plane side of this glass had acquired in the polishing (accidentally perhaps) a very small degree of convexity, probably equal to that of a lens of several feet focal length. This lens, when the plane glass was laid thereon, produced a larger set of primary rings than could be otherwise procured. Its concave side had been ground on a sphere of about two inches radius; therefore the set of transmitted rings, reflected from its lower internal surface, was too small to cause any confusion either in the primary set or its reflected image; and it was in a great measure owing to this accidental circumstance, that these parallel lines happened to be discovered at all; nor could they have been discovered, even with this apparatus, without the use of the shadow.

Exp. 2. Having ascertained the reality of these parallel fringes, I painted the concave side of the plano-concave lens black, in order to prevent all reflection from its concave surface; by which means the fringes were seen in much greater perfection. They were found to consist of all the prismatic colours; were equidistant and parallel; equal in number to the rings of both sets taken together, exclusive of the central one; and each fringe was drawn through the several inter-

sections of the primary set of rings with those of its reflected image; and their lengths extended to the edge of the lens on both sides, they were formed at right angles to the direction of the light, and to a line joining the centres of the primary and its reflected image; which indeed is a necessary consequence of their being projected through the intersections of two sets of concentric rings perfectly equal in dimensions. See fig. 1, pl. VII. compared with fig. 3, pl. VIII.

I was convinced that those parallel fringes consisted of prismatic colours; yet with the apparatus now used, they were too small and too close to each other to enable me to perceive distinctly the order in which those colours were arranged, with respect to each other. As their distances from each other, or, which is the same thing, their breadths depended on the distances of the intersections through which they were projected; it was obvious that by separating, or widening the distances of the latter, the parallel fringes would also be separated, and consequently their breadths enlarged.

Exp. 3. For effecting this purpose, two modes presented themselves, either by increasing the dimensions of the primary set of rings, and consequently that of its reflected image; or by lessening the distance between them. The former mode was at that time out of my power, having no lens of a longer focus than the one in use. The latter was effected by procuring a thinner piece of plate glass than that used in the 1st experiment; and although by this apparatus the field of view was narrowed, the breadths of the fringes were enlarged, and by this means it was easily perceived, that each fringe was composed of the same prismatic colours as the Newtonian rings,

and were placed in the same order with respect to each other; that is, beginning with red, next orange, then yellow, green, blue, indigo, and lastly, violet: but that the fringes themselves were divided into two classes, (with respect to the position of the colours of each particular fringe) by a central band or fringe passing through a point bisecting the distances between the centres of the primary set and its image, each fringe, on both sides of this imaginary point, respecting it as a centre, and having the red colour of each fringe turned *outwards,* or from that centre; the central band alone being equal on both of its sides with respect to colour; being as it were composed of two of the inside halves of the two adjoining fringes, imagined to be laid together; having no red in its composition, and being rather less intense in colour than any of the others. An imitation is given in fig. 1. pl. VII. and as it would be impracticable to express all the colours of each fringe by shadowing, the darkest shade denotes the violet, the unshaded side the red of each fringe, and the black lines the divisions between them.

Exp. 4. Another mode of producing those fringes was by applying a convex and a concave lens together, the curvature of the one differing but little from that of the other. This apparatus produced a larger set of primary rings, and consequently broader fringes, than by either of the foregoing experiments; but in this case none of them were straight, except the central fringe, all the rest being bent more or less into elliptical shapes, conformable to the surfaces between which they were formed.

Exp. 5. But a better mode still was found, by applying a slip of looking-glass plate to one of the sides of a triangular

prism, about $5\frac{1}{2}$ inches long, and a full inch broad; for although the sides had been intended to be made perfectly flat, they had acquired, in polishing, such a small degree of convexity, as on application of the flat glass plate, produced a larger set of primary rings than could be had by any of the former experiments. This apparatus was still farther improved, by painting two of the sides of the prism black, so as to exclude all extraneous light; by which means I could easily perceive that those fringes (which were produced by rings of about three quarters of an inch diameter,) extended from one end of the prism to the other, that is, to an extent of at least seven or eight times the diameters of the rings, and it is uncertain how much farther they might have extended with a longer prism. They were also seen parallel to the edges of the prism, by carrying the eye in that direction, either to the right or left.

Nor do these rings or fringes cross each other undisturbed, for the prismatic colours of the rings, where they intersect each other, are deranged from their natural order; and the division lines of the fringes, where they intersect the other intersections, are drawn into a zig-zag form, but beyond those intersections they proceed in straight lines. These several intersections produce a most beautiful appearance of chequer, or rather net work, the meshes of which assume the hexagonal form, resembling the cups of a honeycomb; of which it is impossible to convey an adequate idea without seeing the experiment.

Exp. 6. Having discovered unexpectedly these phenomena from one set of primaries and its reflected image, I was induced to try what effect could be produced by two sets of

primaries brought into a similar situation with respect to each other. For this purpose, a double convex lens, of about thirty-six inches focus, was laid on a flat piece of looking-glass plate, having its under side painted black; on the lens was placed another piece of plane glass plate; by these means two sets of primary rings were produced, whose positions with respect to each other could be varied at pleasure. On using the shadow of the black card I was agreeably surprized to find, that instead of parallel *fringes* a new species of prismatic *rings* appeared, whose number and sizes varied with the positions and distances of the two sets of primaries; their dimensions were from two to three times the diameters of the primaries from which they appeared to originate, sometimes only one set appeared, sometimes two, and at other times a third very faintly.

On first observing these new rings, it was found, that on moving the eye in a horizontal direction to the right or left, they sometimes moved *with*, and sometimes contrary to the motion of the eye; others were stationary, although the eye moved; also, that sometimes the prismatic colours were seen in the usual order, and at other times inverted; all of which facts seemed not a little perplexing at that time; but their causes will be better understood from what follows.

Exp. 7. By a subsequent experiment, it was discovered, that those rings towards the circumference of the new sets had their colours always in the usual order; but that those nearest the centres had their colours always inverted; that the number of rings of each class were equal: that they all passed through the several intersections of the two primary sets of rings with each other, from which intersections they

seemed to originate; that the dividing ring between the classes, passed through a point, whose distance from the centre of each primary set was in proportion to its largest diameter.

This will be better understood by referring to fig. 2. pl. VII. where A and B are the primary sets of rings: C will represent one of the newly discovered sets, which were denominated *intersectionaries** from their apparent origin. The fourth ring from the centre will be the division between the two classes. Those rings within the division having the red on their *insides*, and those without having the red on their outsides, as represented by the figure; where the same rule has been observed in shadowing these rings, as was observed with respect to the parallel fringes in fig. 1, namely, that the shaded side represents the violet, and the unshaded side the red of each ring, and the dark lines the divisions between them.

Admitting that parallel fringes are necessarily rectilinear, in consequence of being drawn through intersections of circles that are perfectly equal in dimensions, it follows, that where two sets of circles differ in dimensions, the corresponding intersections cannot lie in straight lines, but must necessarily be circular, as will appear evident on inspection of the figure; where the dividing ring D E could not pass through the several intersections in the points 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, unless it were circular: the same thing will hold true of all the other rings of the intersectionary set.

It also appears by the foregoing experiments, that rectilinear fringes and intersectionary rings are coloured exactly alike, and are alike divided into two classes with respect to the order of the colours. There is also an exact similarity in

* The best apology for using a new word is, that it expresses a new idea.

the reticulated crossings before mentioned, whether the sets of primaries by which they are caused, are equal or unequal in dimensions. Fringes and intersectionary rings, therefore, differ in no other respect than as right lines and circles; it is, therefore, a fair conclusion, that the phenomena of rectilinear fringes, formed between two sets of rings of equal magnitudes, are bent into rings when the two corresponding sets are unequal in magnitude.

There is also an infinite variety in the dimensions of those intersectionary rings, according as the diameters of the primaries differ more or less, being *least* where that difference is *greatest*, and increasing in size as the two sets of primaries approach to equality, until at last they end in straight lines. The dimensions of these intersectionaries will also (*cæteris paribus*) diminish as the two sets of primaries approach each other, and enlarge as these are separated; and, like Newtonian rings, enlarge or diminish with the less or greater elevation of the eye. It will be easy to conceive from inspection of fig. 4, (pl. VIII.) that where an intersectionary set of rings is formed between two sets of primaries of unequal magnitudes, it must necessarily appear on the side towards the smaller of the two primaries; which agrees with actual experiment.

The above hypothesis accounts for one set of intersectionaries; but experiment shows that if one set appears, it is almost always accompanied by a second, sometimes equal, but oftener unequal in dimensions, as in fig. 3, (pl. VIII.) at other times part of a third set appears as segments of circles only. It would, therefore, seem that they are formed not only between primary sets, but also between primaries combined with either

transmitted or reflected sets, provided the two, between which they are formed, are unequal in dimensions.

Exp. 8. The four sets of rings represented in fig. 3, (pl. VIII.) were produced by an arrangement consisting of two convex lenses laid on each other, and a piece of plane looking-glass plate laid on the uppermost lens. The large imperfect set, whose centre is at D, was evidently produced by the intersections of the two primary sets, A and B, (the former was observed to have a dark and the other a light centre), but the set whose centre is at C cannot be accounted for on the same principle; but may be owing to another cause, which will be better understood by a future experiment.

Notwithstanding these intersectionaries seem to owe their origin to a few rings only, adjoining that which divides the two classes; yet they are always filled up to the centre with prismatic colours; unless when very large, as that whose centre is at D, (fig. 3. pl. VIII.) or when they appear in part only, as segments of circles; in which case the central spaces appear blank.

Exp. 9. It is perfectly certain that these intersectionary rings are always formed between the two contiguous surfaces of the two lower pieces of glass when three pieces are employed; this is easily proved by the test of the shadow. But if four or more pieces are laid on each other, the number of spectra may be increased indefinitely, and these again may be multiplied by internal reflections; so that to pursue these phenomena through all the varieties of which they are capable would be an endless task.

Let it be observed here, that the several drawings of pris-

matic rings in figures 1, 2, 3, (plates VII. VIII.) are such as would appear to the eye, could it see them at right angles to the plane in which they are formed; but as the eye cannot well see them in this position, nor until it is lowered to an obliquity of about thirty degrees, the proper allowance must be made; for such rings will always appear of elliptical forms, less or more elongated according to the greater or less elevation of the eye. See fig. 4. (pl. VIII.)

Exp. 10. Finding from the foregoing experiments, that two sets of rings of equal magnitudes produced straight fringes, and that those of unequal sizes produced rings; it was obvious that both fringes and rings might be considered as diagonals to the angles in which they were formed. It was, therefore, concluded from analogy, that if primary fringes could be produced between two flat pieces of looking-glass plate, and that if two of those sets were made to cross each other, a set of straight or rectilinear fringes should be formed diagonally between them; and, on making the experiment, I had the pleasure to find the result precisely answered expectation. So far theory and experiment agreed (see fig. 6, pl. VIII.); but in the course of making this and some of the succeeding experiments, several unexpected phenomena were discovered.

Exp. 11. The slips of plate glass used in this and several of the following experiments were from four to six inches long, and about one and a half inch broad, their edges having been ground straight.

On wiping two of those glass slips perfectly clean with soft shammy or a soft dry linen rag, (which is absolutely necessary, as the smallest particle of dust, scarcely perceptible even with a microscope, interposed between the slips,

would prevent the success of the experiment), and by applying two of their ends together, and by using some friction and a considerable degree of pressure, a beautiful set of rectilinear prismatic coloured fringes appeared across the glass slips, having all the prismatic colours in the same order as Newtonian rings, and equally vivid, (see fig. 5, pl. VIII.) and although the breadths of those fringes could be increased or diminished by the greater or less degree of pressure (which had the effect of diminishing or increasing the angle formed by the planes), yet their breadths continued to be uniform, or as nearly so as might be expected, considering the unavoidable imperfections of the best plate glass.

It was proved by Sir Isaac Newton, that primary prismatic rings were repeated at equal increments of interval between two spherical surfaces, because the semi-diameters (or, which is the same thing, the diameters) of those rings were found on actual measurement to be to each other as the *square roots* of the series, 0, 2, 4, 6, 8, &c. beginning at the centre of the rings or point of contact, as must be well known to mathematicians; it might, therefore, have been known by reasoning *a priori*, that if similar prismatic phenomena could be produced between two flat planes, the corresponding repetitions must take place at regular and equal intervals from the point of contact, or vertex of the angle, corresponding to the natural numbers of which the measurements of the rings express the square roots; that is, as the series 0, 2, 4, 6, 8, &c. The principle is, therefore, fully confirmed by this experiment.

Exp. 12. Those primary fringes have also transmitted sets between them, alternate in colours to their primaries, exactly similar to Newtonian rings; for, by placing them longitudi-

nally in the direction of the light, and by using the shadow of a black card thrown across them, both primaries and transmitted fringes may be seen at once by the naked eye. Let the light come in the direction AB in fig. 5, pl. VIII. and let CD represent the edge of the shadow next the light, those fringes above the line CD are primaries, and those below it are transmitted sets, alternate in colours to the others. Although these primary fringes are uniform in their breadths, they are not quite so with respect to colour; for the yellow, blue, indigo, and violet, are predominant in the first, second, and third fringes next to the place of contact, although these four colours are but little perceived after the third fringe, where ten are seen, the red, orange, and green being predominant in all the rest.

Exp. 13. After having produced one set of primary fringes, I succeeded in having another set formed at right angles thereto, by applying a third slip of glass longitudinally to the upper one of the first two, on which the expected diagonal fringes immediately appeared in the angle between the two primary sets, as shown in fig. 6, pl. VIII. where B and C are the primary fringes, and D the intersectionary set divided into two classes, as shown by the dotted line.

Exp. 14. It was found by trials, that the relative position of two sets of fringes, when formed by three slips only of glass plates, connected together as in Experiment 12, could not be changed at pleasure; but, by using four slips, and having a set of fringes formed between each pair, which were unconnected, these could be placed the one over the other in any position, and at any required angle; by this arrangement, it was discovered, that whatever was the magnitude of the angle

formed by the two sets of primaries, that angle was always bisected by the central band of the intersectionaries. And although these latter evidently proceed from the crossings of the two sets of primaries, yet they are never continued through those crossings to the opposite angle at A, fig. 6, (pl. VIII.) nor could they be made to appear in any angle formed by primary fringes, unless the said fringes were so disposed as to have their red sides turned inwards, or towards each other. This remarkable fact was proved by several repetitions of the same experiment.

Exp. 15. In the course of making these experiments, I had accidentally left a single slip of glass on one of the pairs between which a set of primaries were formed, without any other pressure than its own weight; on examining the apparatus a few minutes afterwards, I was agreeably surprised to find no less than four sets of the same kind of fringes which appeared in the angle between the two sets of primaries, as before related in Experiment 12, almost parallel to each other, and nearly so to the primaries, and at about an inch distant in front thereof. (see fig. 5. pl. VIII.) The set marked 2, had broader fringes, more vivid in colour, and consisted of a greater number, (from fifteen to nineteen), than those sets on each side thereof; for these latter appeared to have that inferiority of colour to the principal set, that it bore to the primary set. All the four sets were by attentive observation visible to the naked eye, and were all divided into two classes by a central band, as was before observed of the others; and on application of the shadow, were found to consist of primary and transmitted fringes, precisely in the same manner as first primaries, or Newtonian rings.

A representation is shown in fig. 5, (pl. VIII.) before referred to, where the sets denoted by 1, 2, 3, and 4, may be called primaries in respect of 5, 6, 7, and 8, which, in like manner, may be denominated transmitted sets; all the space below the line CD being supposed covered with a shadow of a black card.

Not having hitherto observed these secondary fringes, except where they proceeded from intersections of primaries, it was concluded, that this was the only cause of their appearance; but, by this last experiment, I was convinced of the error of this opinion, and that they are entitled to the same rank of originality as primary fringes or Newtonian rings; and, since they are always found divided into *two* classes, I shall henceforward venture to denominate them *binaries*, which perhaps may not be an improper characteristic for the whole genus, in contradistinction to *primaries*, as it will appear in the sequel, that there are several species of the former, and at least two of the latter.

Exp. 16. This experiment was made to ascertain whether two slips of glass only, when the uppermost was pressed by its own weight alone, would produce binary fringes between them; and I succeeded in perceiving one set whose breadths were about one-tenth of an inch each, see fig. 9, (pl. IX.); these are best seen by clear day light; but it is also necessary to have a piece of black velvet under the lower slip, otherwise to have its under side painted black.

Exp. 17. In endeavouring to see these fringes mentioned in the last article by candle light, I was disappointed, but accidentally perceived others of a different species, less in breadth, but much more numerous, and by which the whole length and breadth of the glass slips were covered. These were visible

by the light of a small candle only, nor could they be perceived except in the image of the blaze of the candle reflected from the upper surface of the upper slip of glass, and by moving either the eye, or the candle, or the slips across the direction in which the fringes appeared: and even with every precaution, I sometimes failed in seeing them, which I attributed to the presence of very minute dust between the glass plates, which in some cases can be known only by its effects. Moreover, when the eye changes from a greater to a less quantity of light, it requires some time to adapt itself to such delicate and minute objects, before it can perceive them satisfactorily. See fig. 8. (pl. IX.)

Exp. 18. By subsequent trials it was found, by the use of an oil lamp, having five or six small wicks in a row, composed of hempen packthread, which produced a pale brownish light, that those singular fringes could be seen with nearly as much ease and certainty as any of the other phenomena, though not all at once; but, by moving the light over them, otherwise moving the slips with respect to the light, and by using a magnifying glass, it was ascertained that the specimen represented by fig. 8, (pl. IX.) contained thirty of these fringes to an inch. Being uncertain whether they might not be the same as the primary fringes mentioned in Experiment 11, or a continuation of them, I caused a few of these latter to be formed at one end of the slips at A, fig. 8, (pl. IX.) when it was found that they crossed each other at a certain angle as shown in the figure; therefore, they could not be the same. Moreover, as they have no perceptible colour like first primaries, nor are divided into classes as binaries, they must be different from either.

Exp. 19. It next occurred to try what effect could be

produced by three slips of glass when laid together, and pressed by their own weight alone. Leaving them in this situation about fifteen minutes, I found an irregular set of binaries spread over the surface of those slips; a representation of which is shown in fig. 9. pl. IX.

On drawing or sliding the upper piece of glass along the surface of the middle one, these fringes changed their shape, and disappeared as far as the middle slip was uncovered by the upper one; or if the upper two were kept together, and both moved over the surface of the under slip, the same phenomena took place. But if the upper glass was in the smallest degree separated from the other two, or if the two upper pieces were in the same degree separated from the lower one; in either case, the spectra first changed their shapes and then vanished; but, on leaving the slips to the pressure of their own weight, were again as instantly restored. On changing the position of these three slips by placing the two upper ones across the lower one, a new spectrum was formed as seen in fig. 10, pl. IX. In these as well as in the following figures of binaries, the central band, dividing the two classes, is always denoted by a dotted line.

These spectra may be varied almost to infinity by the smallest change of distance or relative position of any one of the three slips to the other two; and it affords a pleasing amusement to observe those fleeting forms start into new and fantastic shapes in such a manner as strongly to resemble, in miniature, the coruscations of the aurora borealis. In order to determine whether these spectra were formed between the two upper, or between the two lower surfaces of the glass slips, the test of the shadow was applied, which determined

them to be always formed between the two lower contiguous surfaces.

Exp. 20. But it appearing very unaccountable that the mere presence of the upper glass slip should produce spectra between the two lower slips; I wished to have the fact corroborated by another experiment. For this purpose two similar slips of glass were so closely applied together, as to produce primary fringes between them; and in this position they were cemented together with bees' wax, to prevent shifting: this double slip being substituted, instead of the two upper unconnected slips in Experiment 12, produced binaries as usual. On application of the shadow, the primaries appeared in the second, and the binaries in the third shadow, as expected; but it being perfectly certain that the primaries were formed between the two upper contiguous surfaces, and as the shadow proved that the thickness of one slip of glass was interposed between the two spectra, there could not remain a doubt of the binaries being formed between the two lower contiguous surfaces.

Exp. 21. These results being so unexpected with three slips, it naturally occurred to me, to try what effect four would produce. Having placed two, laid together horizontally on a table, I took up the double slip mentioned in Experiment 18, in order to clear it of dust, and was in the act of holding it up between the light and the eye, in order to examine whether it was perfectly clean; and it happening accidentally to be in the direction of the two lying on the table, I was agreeably surprised to find a spectrum already appear, as if formed in the air, although the two pairs of slips were several inches asunder; which spectrum, on moving the

double slip in the hand, out of the direction of those on the table, vanished as far as those on the table were uncovered by those in the hand. And notwithstanding this spectrum appeared to be formed in the air, the test of the shadow proved it to be formed between the two slips on the table; and although it was visible when the two pairs of slips were not less than ten inches asunder, it could not be perceived except through the medium of the double slip held in the hand.

A representation of this experiment is shown in fig. 11, pl. IX. where AB is a section of the two slips held in the hand; and CD of those on the table. Fig. 12 pl. IX. represents one of those fleeting and fantastic forms as seen by the eye at F in this experiment.

Exp. 22. If the result of the last experiment appears extraordinary and unaccountable, the present one will appear still more so; for, on lowering the double slip in the hand from its former elevation of about 45 degrees, to about 15 degrees, so that its image was seen reflected from the upper surface of the upper glass on the table, as represented by GH in fig. 13. pl. IX., a new spectrum, fig. 14, pl. IX. was seen in this reflected image, superadded to that shown in fig. 12, which was not to be perceived in the object A B itself! Both spectra together, as seen by the eye at F, are represented in fig. 15.

Exp. 23. It was fully ascertained by the test of the shadow, that while the double slips were in the position described in Experiment 20, both spectra were formed between the two slips on the table; but, on lowering the two upper slips until they were laid flat on those incumbent on the table, both

were gradually metamorphosed into two others; one of which was formed between the two lower, and the other between the two middle slips brought into contact by this new arrangement; this was also proved by the test of the shadow. It may be worthy of remark, that in every variety of shape assumed by these spectra, the binary characteristic is clearly visible in them all.

Exp. 24. On viewing a set of Newtonian rings, as described in Experiment 4, through one of the double slips, while in the position fig. 11. pl. IX, the apparatus forming the rings being on the table; a set of narrow binary rings appeared, concentric to the primaries, and near to the edge of the lenses: and on lowering the double slip to the second position, fig. 13. pl. IX., another set of binary rings appeared in the reflected image of the double slip, also concentric to the primaries, but consisting of broader rings, though less in diameter than those of the first set; and all these three sets were seen distinctly, at one and the same time.

The two sets of binary rings, seen in this experiment, correspond to the two irregular spectra in experiment 20; and since the rings must have been formed between the lenses on the table (because no *rings* are ever formed unless between surfaces, one of which at least is spherical), this affords a corroborating proof, that the irregular spectra seen in Experiment 20, although apparently formed in the air, were really formed between the two slips of glass lying on the table.

The above results differ so widely from any that have hitherto been published, that it is allowable to doubt, whether they can be accounted for on the common and received

Fig. 1.

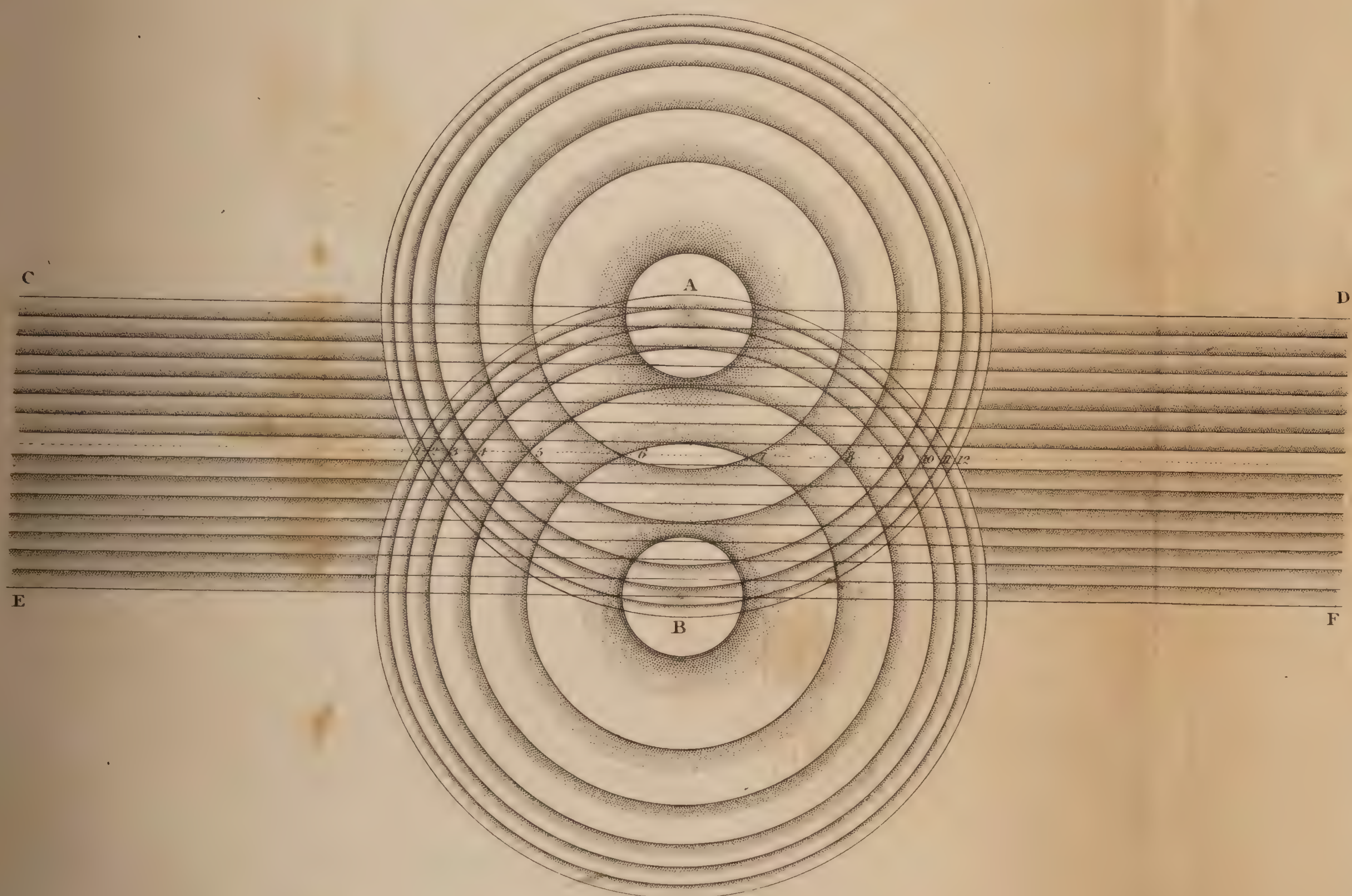


Fig. 2.

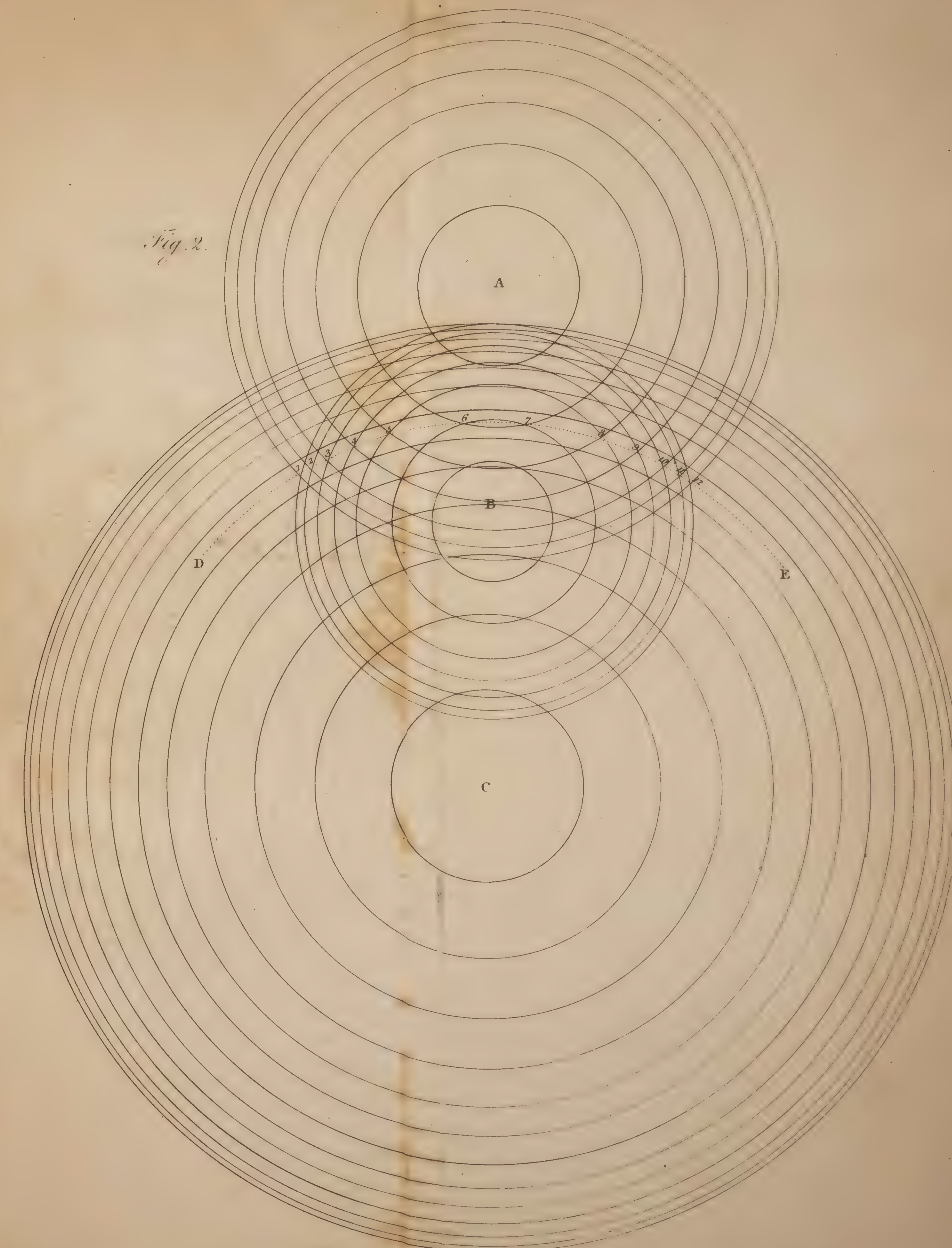


Fig. 3.

Fig. 4.

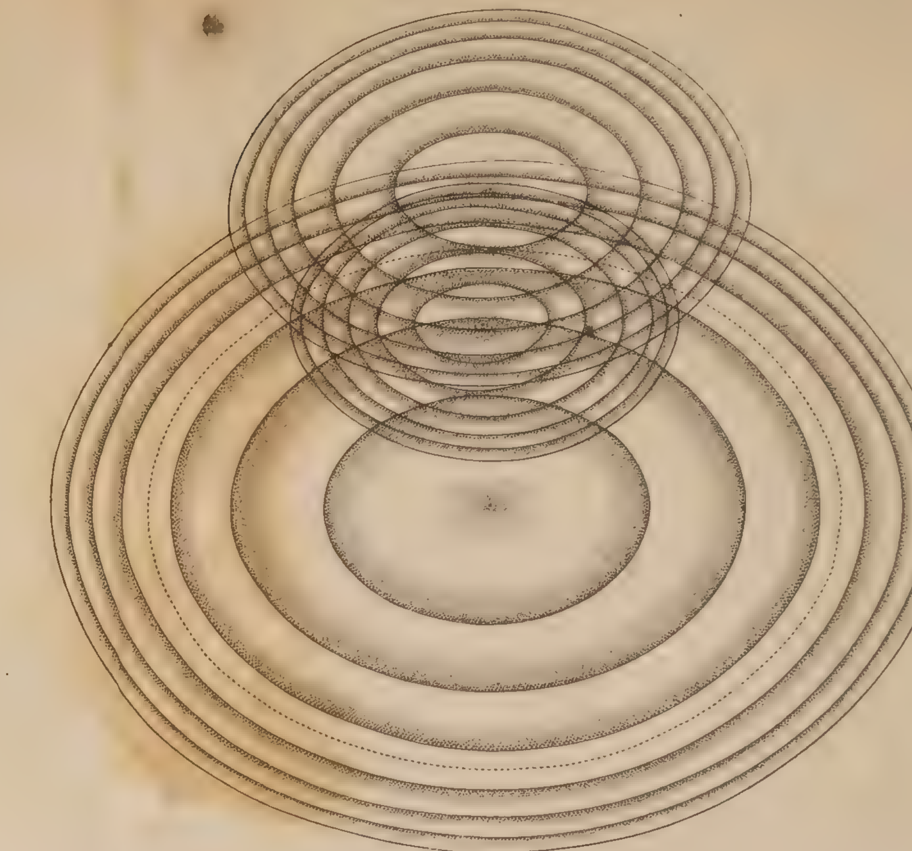


Fig. 5.

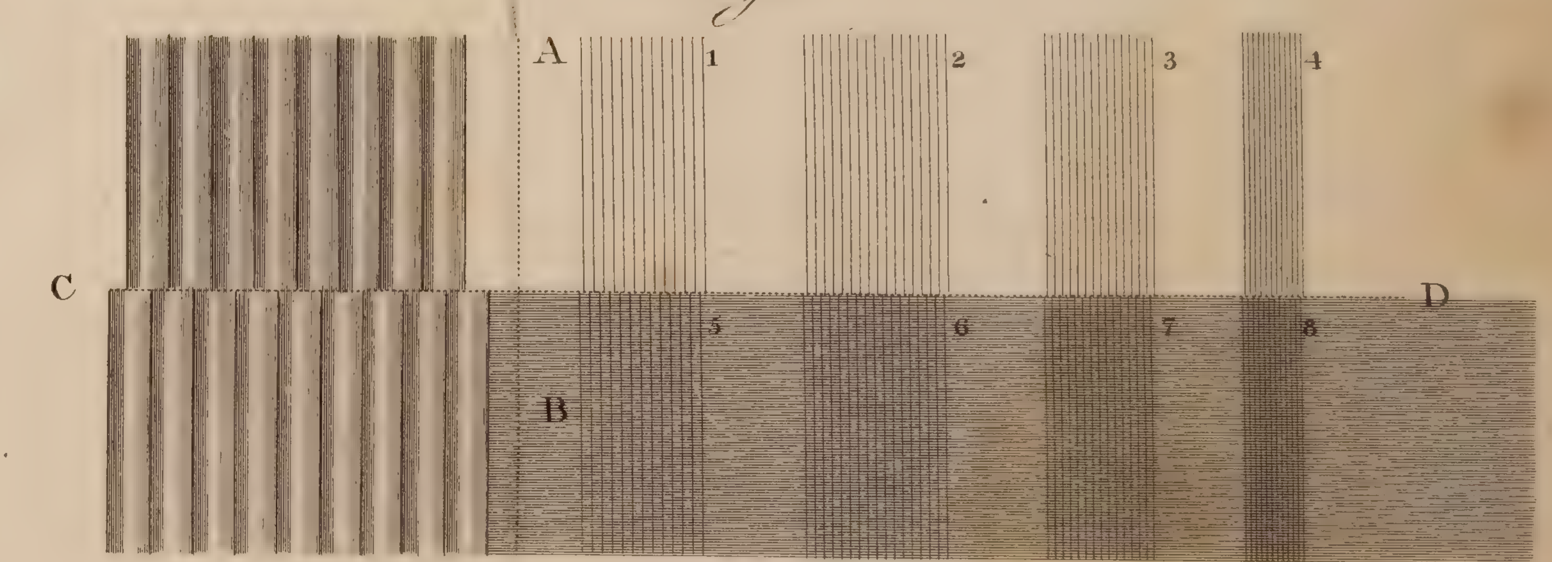
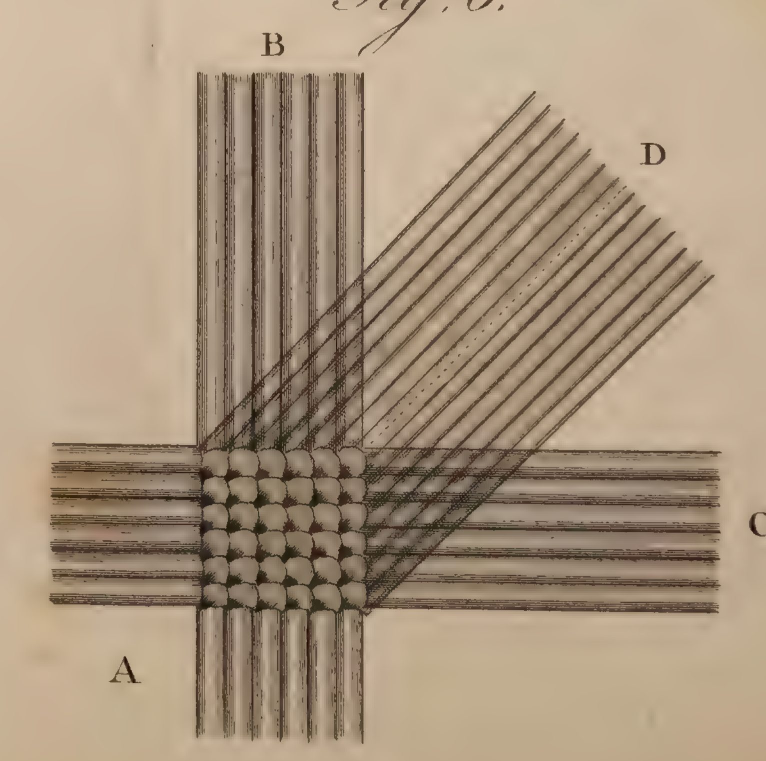


Fig. 6.



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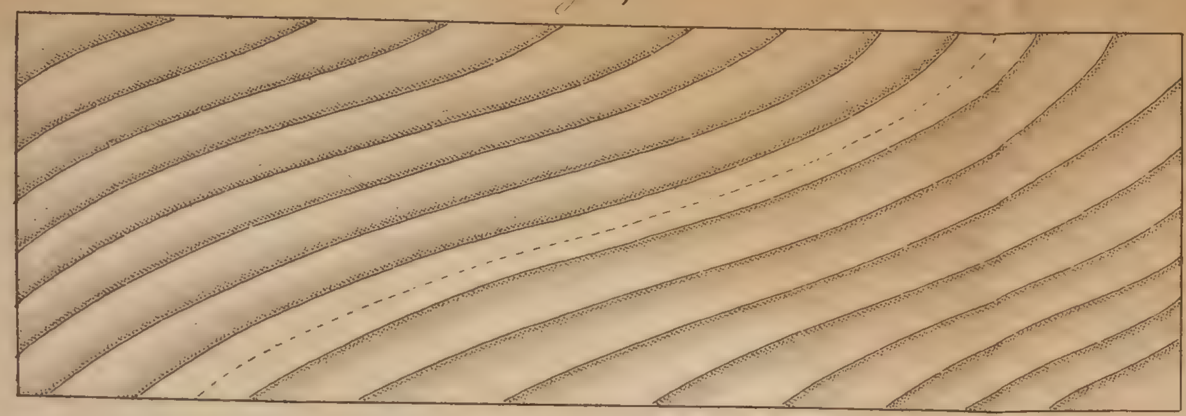


Fig. 7.

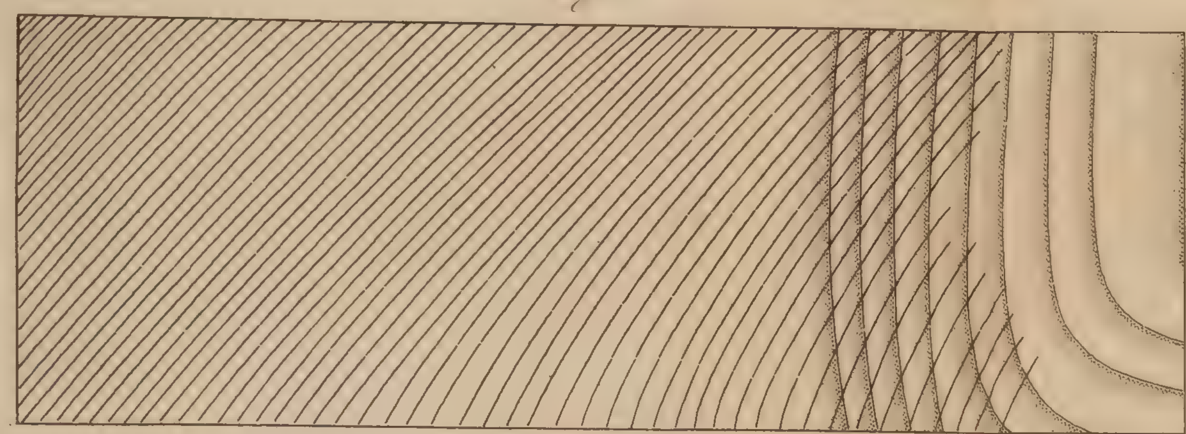


Fig. 8.

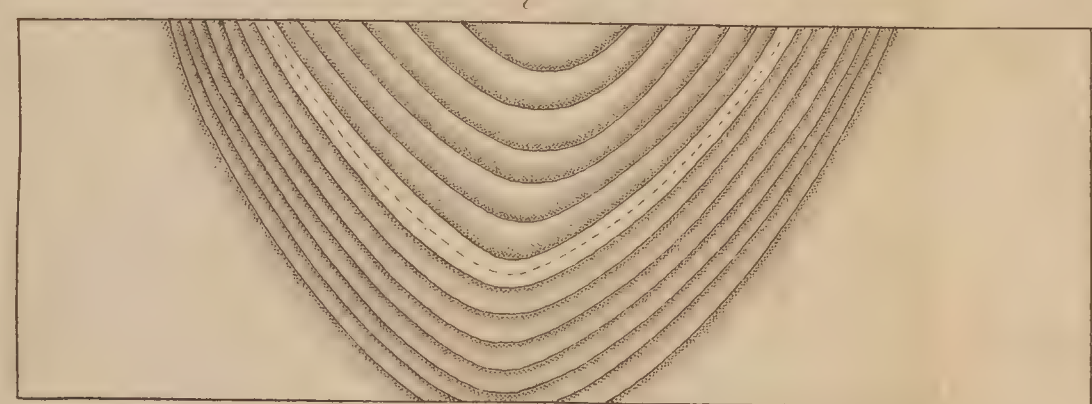


Fig. 9.

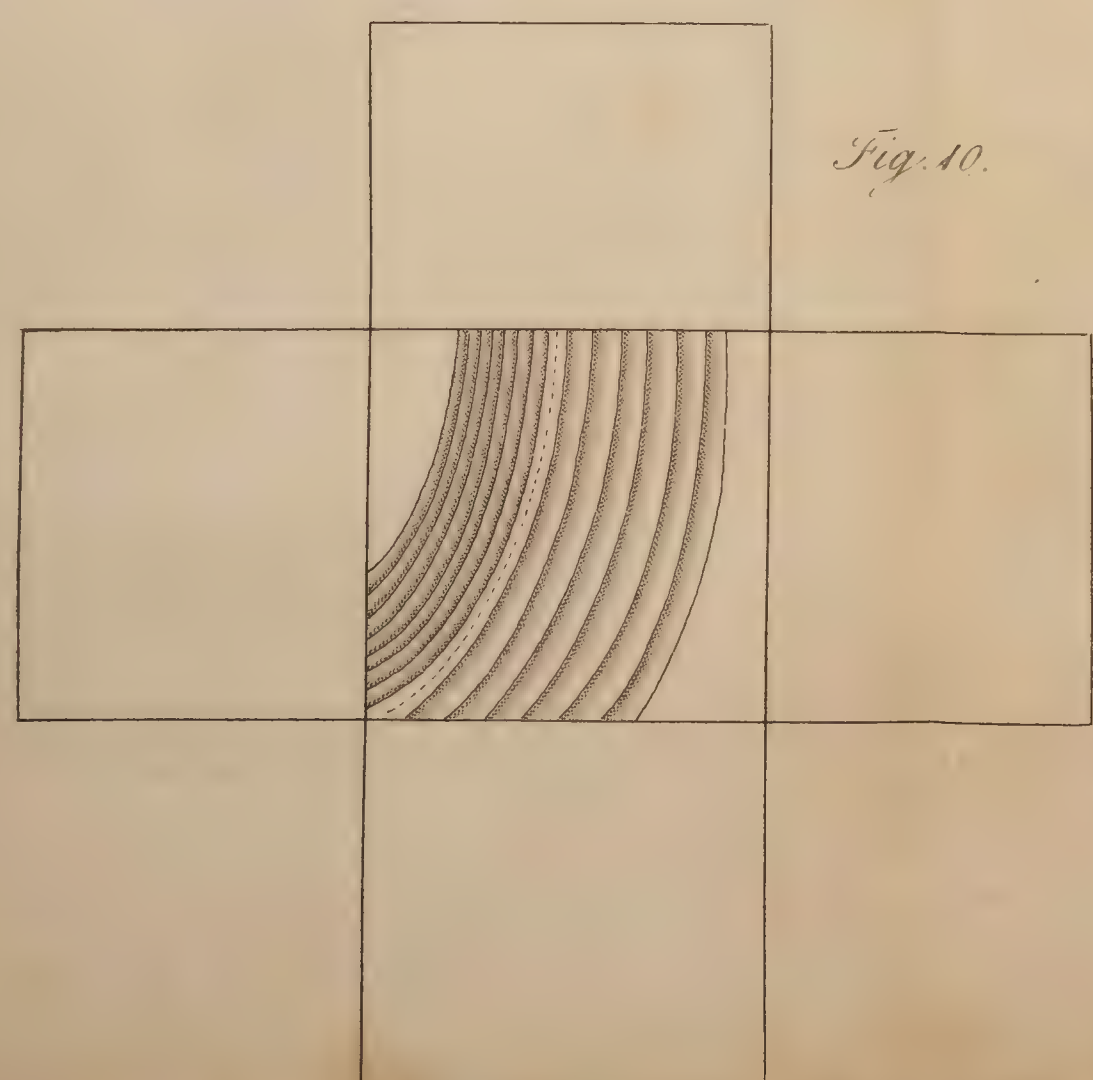


Fig. 10.

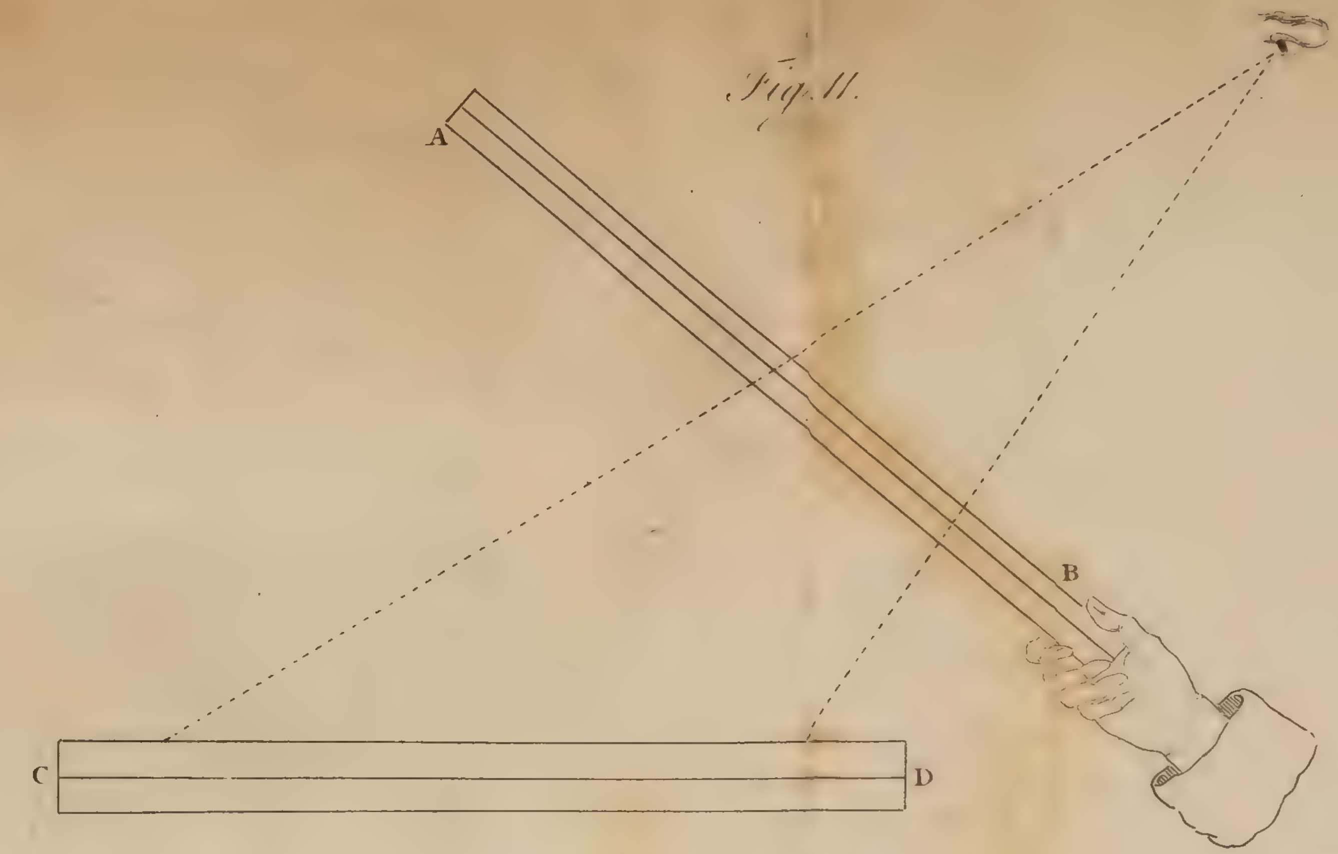


Fig. 11.

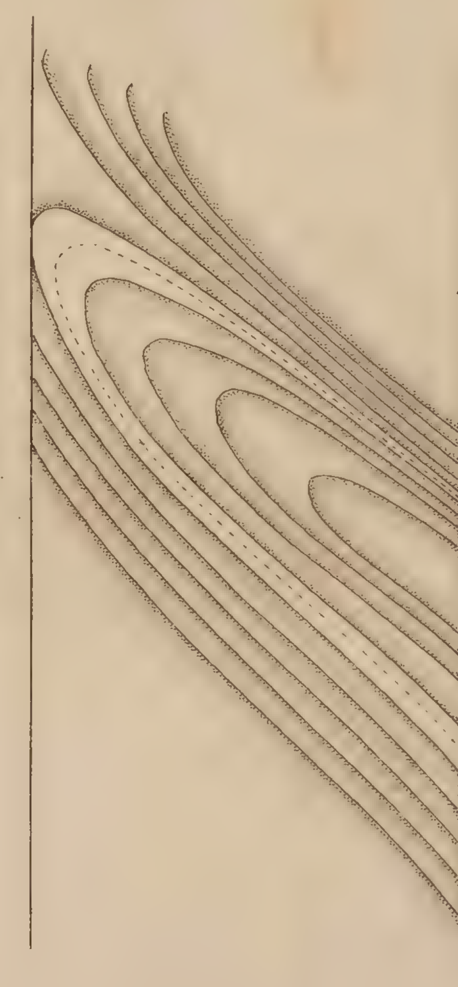


Fig. 12.

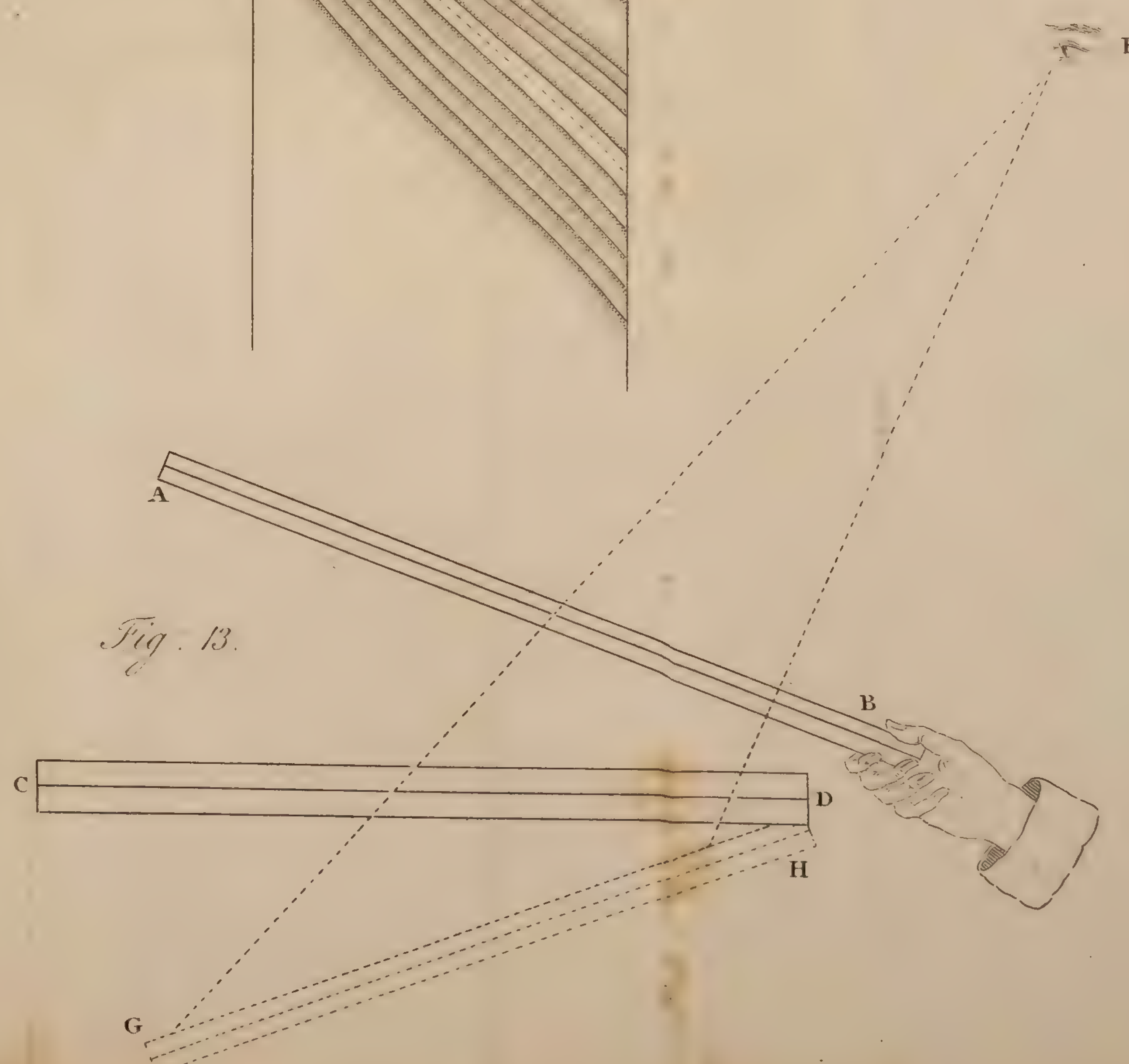


Fig. 13.

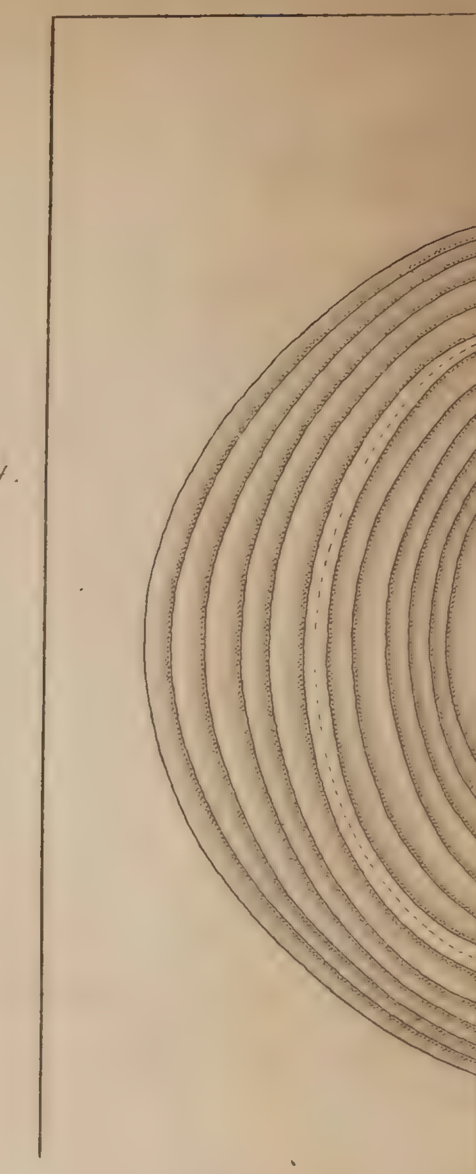


Fig. 14.

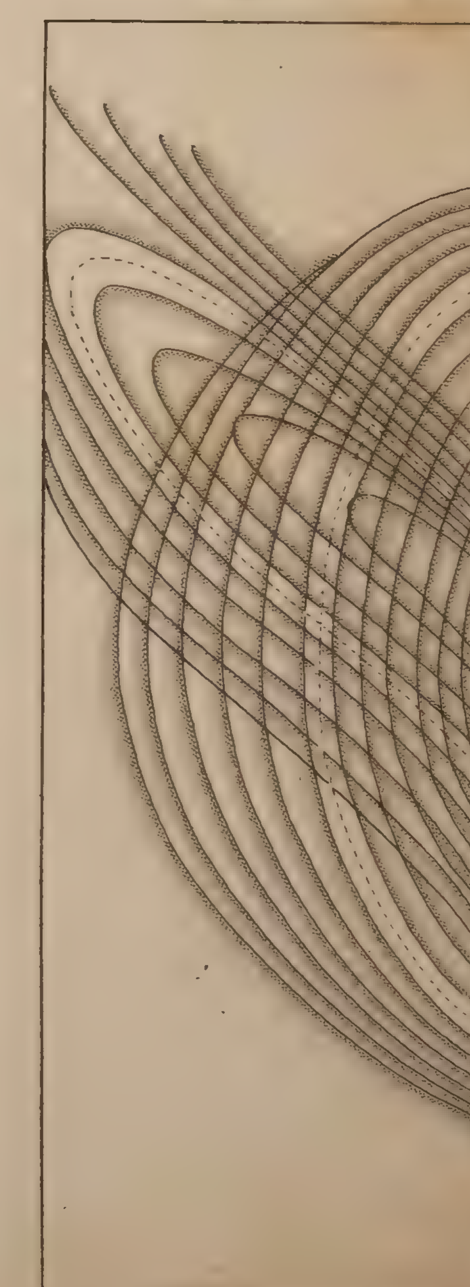


Fig. 15.

principles of physical optics, unless perhaps the newly discovered principle, which has been denominated the *polarity* of the rays of light, may serve to explain their causes. However, " as every desideratum may be considered as an imperfect discovery," which when completed may lead to others of still greater importance; and as the subject is so far from being exhausted, that perhaps we are only entering on a new career of discoveries, not only in optics, but in every other branch of natural knowledge; in this point of view, the prospect before us is rendered as extensive as it is animating and delightful.

JOHN KNOX.

Belfast, Oct. 6, 1812.

XI. *Some farther observations, on the current that often prevails, to the westward of the Scilly Islands. By* James Rennell, *Esq. F. R. S.*

Read April 13, 1815.

During the interval of 21 years, since the Society did me the honour to receive my Observations on the Current to the Westward of Scilly, more facts relating to that current, have been collected; as well as observations on its effects, in different parts of its course, between Cape Finisterre and Scilly: the whole tending to a confirmation of the general system set forth, in 1793; and, in one instance, affording, perhaps, a clearer proof of the strength of the stream, in respect of its *northerly* direction, than any of those, adduced on the former occasion.

In pursuing the detail of these facts and observations, I shall begin, in the neighbourhood of Cape Finisterre, and proceed with the course of the current, along the Bay of Biscay; and thence, across the mouth of the British Channel, to Scilly, and the entrance of St. George's Channel.

The three first facts, regard the current from the open sea, setting into the south side of the Bay of Biscay, and along the north coast of Spain; which current has been supposed, in the former Paper, to be occasioned by the prevalent westerly winds; which force the water near the shore, *into the Bay*, and along the southern coast of it. The water so displaced, would be followed of course, by the adjacent water *behind* it,

in the open sea; and so on, successively, to a certain extent. This cause, and not the effect of the *Gulf Stream*, extended to the coasts of Europe, as some have supposed, must surely be referred to, as the origin of the Scilly current.

I. The first case, is that of the Earl Cornwallis East India ship. The circumstance occurred on her outward passage: she was well provided with time keepers, as most of the India ships are.

On the 12th March, 1791, between the parallels of 43° and 44°; and at 3° 45′ of longitude, west of Cape Finisterre, (about 53 leagues), this ship experienced an easterly current, equal to 26 marine miles. Her position being directly opposite to the line of the southern coast of the Bay of Biscay, it is a fair conclusion, that the current was occasioned by the cause abovementioned; or as seamen call it, the *indraught* of the Bay: which, it appears, extends to, at least, 53 leagues from the shore. And as the rate, in this place, exceeds one mile *per* hour, it may be supposed, that the effect extends to a still greater distance.

It may here be remarked, that the same ship, in coming out of the *Chops* of the Channel, a few days before, was *set* twenty four miles to the westward, 15 to the northward; in the course of the 24 hours: that is, 28 miles, in a direction of N. W. by W. This may be supposed to be the same stream of current, in its course from the *Bay* towards *Scilly*.

II. The second fact, is that of the *drift of a bottle*, which was thrown out of a Danish ship, (I believe, sent on discovery) since the publication of the former Paper.

The bottle was thrown out, in lat. $44\frac{1}{2}°$, lon. 12° west from Greenwich: that is, about 48 miles to the N. E. of the Corn-

wallis's station, at the time that she began to feel the current, on the 11th March. It was taken up by a centinel on duty, near Cape Ortegal; and, as was supposed, at the moment of its driving into the surf. If this was really the fact, the bottle, according to the date of the letter contained in it, must have been carried, at the rate of half a mile *per* hour, in the direction of about E. b. S. $\frac{1}{2}$ S.; the distance was about 64 leagues.

The report of this circumstance was transmitted by the French consul at Corunna, to the Academy of Sciences at Paris.

It may be observed, that the drift of the bottle was much to the *south* of east; whereas, that of the Cornwallis, was east: that is, both pointed towards Cape Ortegal, or its vicinity; as if the main stream of the current, was concentrated there.*

With respect to the velocity of the current, in the present case, all, of course, depends on the time of the arrival of the bottle at the shore. It might have been thrown up long before it was seen, and washed off again, by the tide, or surge of the sea. The *direction*, the most important point, cannot be questioned.

III. The third fact is very simple, and perfectly conclusive. Off Cape Ortegal, at a considerable offing, Admiral Knight found the current, at the rate of one mile *per* hour, setting to the E. S. E.; that is, nearly *along-shore*.

The reader will immediately perceive that these three

* It is observed, that, in the mouth of the Strait of Gibraltar, between C. St. Vincent and C. Cantin, the currents point in all directions, between SE. and NE. towards the entrance of the Strait, which may be considered as the pipe of a funnel.

facts, converge, as it were, to one point: that is, in the proof, that the waters of the Atlantic flow into the Bay of Biscay, along the north coast of Spain.

It would seem that the north-westerly current, by Scilly, did not, at least in many cases, balance the easterly current round Cape Ortegal, and the land of Finisterre.* The loss of His Majesty's frigate, Apollo, with most of her convoy, may surely be attributed to the operation of this current. Captain (afterwards Commissioner) WALLIS, assured me, that after having made, as he supposed, ample allowance for clearing Finisterre, yet, in the night, he had a very narrow escape from shipwreck. Very many others have been brought into the same kind of danger: so that the land of Finisterre, were it not discernible at a considerable distance; and its offing clear of rocks and shallows; and moreover, situated in a finer climate, would prove a kind of Scilly, to mariners.

I have not been able to obtain any proofs, on record, concerning the course of the current, *round* the Bay of Biscay. I formerly collected some information from a French commander respecting it. He said, that the setting of the current along the coast of France, to the north and north-west, was a fact well understood; and even acted on by many, in the choice of the *tack*, on which the current gave the greatest advantage, with dead winds.

One circumstance, and that a very striking one, in respect

* Nor, admitting an equal rate, in both places, could it well be. For the current enters the Bay of Biscay, in an *east* direction, but goes off from it, *north-west*. So that, if a ship was carried fifty miles to the NW. from Ushant, she would only have made about thirty five westing: but in the other case, she would be carried the *whole* fifty, eastward towards the Bay, and Cape *Finisterre*.

of this particular, is, that the soundings in the Bay of Biscay show little, or no *muddy bottom* to the *southward* of the *Garonne* river; but every where to the *northward.* This seems to show that the mud of the *Garonne*, *Charante*, *Loire*, &c. &c. is all carried to the northward; and by what cause, but a northerly current? Had the motion of the sea been variable, the mud would surely have been distributed, to the south, as well as the north, of the mouth of the Garonne. The alluvial *embouchures* of the rivers in general, here, and the positions of the banks formed by them, in the sea, point to the N. or NW.; apparently the effect of the same sea current.*

IV. In continuation of this current, along the Bay of Biscay, I shall next mention, that Captain (afterwards Admiral) JOHN PAYNE assured me, that being in His Majesty's ship, Russell, in a severe gale of wind at SW., and with the ledge of rocks called the *Saintes*, not far to leeward, he was under apprehensions for the safety of the ship, during a whole night: but to his surprise found himself carried clear of the danger, by a current; which set the ship, in all, about seventy miles to the north-west.

V. The flowing of the tides, on the west of Scilly, cannot well be accounted for, on any other supposition, than that the flood is prolonged by a southerly current. The flood tide is

* From a view of the chart of soundings, between Spain and Ireland, one might be led to suppose, that the deep water and steep shore, along the north coast of Spain, had been *partly* occasioned by the water driven in from the Atlantic, in westerly storms, along that coast; and which had gradually worn away the matter *there*, and deposited it on the bank, which extends from Bayonne to the westward of Ireland. For the bank seems to expand, as it goes northward, in like manner as the current: and the water is shallower than might be expected, in proportion to the depths, farther in.

known to run nine hours to the northward; but the ebb, in the opposite direction, only three hours. This particular had not come to my knowledge, when the Paper of 1793 was written.

VI. But the most satisfactory proof, not only of the *existence* of a *northerly* current, athwart the mouths of the British and Irish channels, but also of its *velocity* (at least during certain intervals), is a statement in a book published in 1733, entitled JOSHUA KELLY's Treatise of Navigation,* (in two volumes octavo). This case is the more satisfactory, as it happened in a *dead calm* of forty-eight hours continuance: so that all uncertainty, regarding the accuracy of a *sea reckoning*, allowances for *leeway*, *drift;* &c. is precluded; since the changes of position that took place, could only have been effected by the motion of the sea, either in the nature of a *current*, or of a *tide:* and this latter must be placed out of the question, since the interval of time, included no less a space than that in which four *fluxes*, and as many *refluxes* have their periods: so that they may well be supposed to balance each other.

" It has been observed (says Mr. KELLY)† by an experi-
" enced commander, who used the West India Voyages for
" many years, from England, (in his return from one of
" these) that in about the latitude of 48° 30′, open with our
" British channel, having a good observation (of latitude) at
" the same time, it proving calm and smooth water, insomuch
" that he handed his sails, and so lay forty-eight hours. The

* This was originally pointed out to me by Mr. JOHN PURDY, the very able hydrographer, employed by Messrs. LAWRIE and WHITTLE, Fleet-street.

† Volume the first, page 434.

" first twenty-four hours at noon, he observed the latitude " again, with clear weather; and found by the same that he " had *drove to the northward twenty miles;* which made him " distrust his former observation, though his mate agreed " with him; because the ship had not gone, to his knowledge, " one mile: and upon review, he found that he was not mis- " taken. The next twenty-four hours, being still calm, he " had again another good observation; and then found him- " self about twenty-six miles to the northward of his last " observation; which confirmed him that he was right, the " day before; and that this must be imputed to a strong " northern indraught, or current, there. For when you come " near the soundings, and till you bring Ushant south of you, " on the E.S.E. course,* *you will hardly hold your latitude;* and " the general course is E. N. E. or E. b. N; if but a small " matter to the southward of latitude 49°. And he says, " that would have been my course, if we had not met this " opportunity of discovering this strong indraught: and " for want of observation [i. e. if he had not known the lati- " tude] must have run up St. George's channel, or the north " channel, as many have, and still do, for want of the same " [information.]

" After his last observation, the wind sprang up; and " making allowance for the said indraught [i. e. in his future " course], the next day he was brought into soundings; and " the following day, he was brought in sight of the *Lizard*, " by steering to the southward of the east.†"

* These are *compass* bearings. The magnetic variation, at that time, being about a point and a half, westerly, these will be respectively E ½ S; NE ½ E; and ENE ½ N; true.

† Meaning, no doubt, the ESE course, by compass, as above, or true E½S.

It will naturally occur to the reader, that although this case gives the *northing only;* yet that, in respect of the main question, which is, the danger of shipwreck, on Scilly; or of being carried into the Bristol Channel; it is sufficient to produce a conviction of the necessity of attending closely to the ship's course, when on the point of entering the British channel, after, or during, a course of strong westerly, or south-west winds. But it would, doubtless, have been more satisfactory, had the *direction* of the stream been known. Had that been *north-westerly*, as I have before supposed, the rate of velocity must have been more than a mile and a quarter *per* hour; or approaching to one and a half (the northing being twenty-three at a mean in the twenty-four hours): whilst that in the Atlas East Indiaman, recorded in a former Paper, was about one mile *per* hour, during four days, consecutively.

The statement in Mr. KELLY's book, which is indeed, altogether, more brief than could be wished, is also defective through the want of the distance sailed, from the place of the last observation for the latitude, to that, from whence they saw the Lizard point. They had their first soundings, the day after that observation; and on the following day, they saw the Lizard. His course appears to have been regulated with a view of preserving nearly, his parallel of 49° 16′; to which he had been carried, by the current. It is not likely that he sounded to any great depth: perhaps seventy fathoms; which in that parallel might have been about twenty leagues south-west from Scilly: and it does not appear that he considered himself in soundings, when the calm began; which

however, it is probable he was, although in deep water.* Accordingly, one may conceive that his position, *at the end of the calm*, might have been about the meridian of Cape Clear, or somewhat to the eastward of it. It must be recollected, that in running towards the channel, after the calm, he had still to encounter the same adverse current: and that, possibly, to within thirty or forty miles of his seeing the Lizard.

But, whether his position, during the time that he was under the influence of the current, be a degree more or less, to the eastward, the fact bears the same on the main question; since a ship, in crossing the stream, wheresoever it may be situated, must have been carried out of her reckoning; and thereby placed in danger; in the event of thick weather happening subsequently, and preventing their setting themselves right, by an observation of latitude.

His idea of the eastern edge of the stream, is worth remarking; as it approaches, in a general view, to the truth. It was, that in *about* the parallel of 49°, it approached to the meridian of Ushant. And with respect to the *direction* of the stream, as he calls it a *northern indraught*, he certainly concluded that it ran to the northward, into the St. George's, or Irish channel; brushing the west side of Ushant, and the Land's End. And the effect of the current, on his ship, was no doubt, such as to warrant that belief, with those whose knowledge of the subject was confined to the mere effect of setting them to the northward of Scilly, and into the mouth of the Bristol channel.

The information contained in this statement, does not even

* Perhaps thirty to thirty-five leagues to the west of *Ushant*, and in about 100 fathoms.

terminate in the mere facts of the existence, and force of the current. The commander of the West India ship, is said to have made *many voyages* to, and from, that quarter; and his narrative shows him to have been *an observant man.* Yet *he* was ignorant of the existence of such a current, until the case occurred, which has been just stated. This then, alone, may serve to show, very satisfactorily, that the current does not exist in strength, but at certain intervals: and therefore operates in a more dangerous, because a treacherous manner.

Had it constantly prevailed, like that round the Cape of Good Hope, &c. it could not have escaped detection; and, in consequence, few, or no evils, would have ensued: but these effects being only felt casually, they were considered as mere contingencies, arising from wind and weather, as in other parts of the sea; and not as resulting from a fixed cause, always operating, although in very different degrees: since no person at that time, had collected the different cases, with a view to examine, and to compare them. Some indeed, referred it to the indraught of the Bristol Channel; without considering, that if such a power existed at all, it was difficult to conceive how it could be suspended; and why it should not operate at all times.

Our navigators, in earlier times, appear to have entered the British Channel, on a more southerly parallel, than they have done in latter times. For, although they might have been ignorant of the real cause of the disturbance in their course, yet many of them believed that there was an *indraught,* as they called it, into the St. Georges's Channel: so that one effect of the current; that is, the *northern set,* had not passed unobserved, although the *cause* was not understood: nor, of

course, could it be known, when to expect it. But I have also heard it remarked by sea officers, as long ago as I can remember, that " it was unaccountable, what should occasion " their *running down so much distance*, in coming in with the " land, from the westward." I never heard, however, that there was any suspicion of a current, setting to the westward.

The idea of a northern *indraught* into St. George's Channel, (but which applies equally to the current west of Scilly) is clearly set forth, in a publication by Captain JOSEPH MEAD, in 1757; but which only came to my knowledge very lately, by the favour of Mr. PURDY; to whom I stand indebted, also, for the knowledge of the important fact of the *set* of forty-six miles, during the calm, in the Chops of the Channel.

Captain MEAD first relates the case of the ship Hope of Liverpool, bound from the coast of Guinea, to that port, in November 1735. (Preface, page iii.)

" Having had a good observation, by which they found " they had the Irish Channel open, the wind continued to " blow strong from between the south and west, but mostly " from the former. Having no other observation [of lati- " tude] for six days, in which time, they carried sail, con- " stantly, they by reckoning expected to fall in with Cape " Clear; but in the following night, they fell in with the " *Blasquets*." These islands and rocks are situated in lat. 52° 10′; or about forty-eight miles to the north, and one degree of longitude to the westward of Cape Clear.

Again (page 10) he says, that the Bristol merchant ships, which fall in with Cape Clear, on their homeward passage [from the West Indies, &c.] shape their course from thence, with a large wind, to the high land, near *Padstow;* which is

the land they choose to make, to lead them to the entrance of the Bristol Channel. That in estimating this course, they allowed four or five degrees in the bearing, to compensate for the indraught into St. George's Channel. This angle would give about thirteen or fourteen nautic miles: and is probably what they found by experience, to be the general amount of the *northern set.**

He goes on to say, that, in like manner, the safety of ships, after they come into soundings, till they reach Scilly, depended on their making *no less allowance*, than the Bristol men do, in the other Channel. For, says he, "experience "informs me, that from the commencement of soundings, in "lat. 49° 30′ N. to the length of Scilly, in *fair weather*, I had "found the northern indraught to be six or eight miles in "the twenty-four hours."

Here then, the fact of the *northern set*, is a second time recognised; though without any suspicion, any more than before, of there being a *westerly set*, also.

Here it may be proper to state, what appears to me to be a very important fact; although perhaps, not connected with the current in question; but materially affecting the safety of the navigation, between the British Channel and Dublin. It was communicated to the Author, by Captain Evans, a gentleman who superintends the harbour works at Holyhead; and who has had much experience in the navigation of the Irish sea.

* Although they might not have known at that day, the *true* latitude of Cape Clear, yet it may reasonably be supposed that they knew the quantity of the *difference of latitude*, between Cape Clear, and the high land of Padstow; as it was so necessary to their purpose, and so easy to be obtained.

All navigators, says he, in their voyage from the Land's End to Dublin, find themselves, more or less, carried to the eastward, whilst running up St. George's Channel: which is the cause of so many vessels finding themselves in Cardigan Bay; where, in tempestuous weather, and westerly winds, many have been lost. And this he justly supposes to be occasioned by a current setting to the north-eastward.

If the stream, which occasions this disturbance in the reckonings of vessels, here, be a portion of the Scilly current, it cannot well happen in any other way, than by the eastern part of that current falling on the Irish Coast, to the *east* of Cape Clear; and being thence diverted to the north-east, along the south-east coast of Ireland. This may certainly happen; and may form a part of the cause. But I conclude that the principal part of the cause, is, a current *generated* on the south-east coast of Ireland, by the prevalency of south-west and WSW. winds; to which, the position of the coast, between Cape Clear and Carnsore Point, seems particularly adapted.

This effect, from whatsoever cause it may arise, ought to be generally known; as it may produce great inconvenience and distress, to those, who for the first time make use of that navigation: and especially to such vessels, as are either not calculated, or not in a state, to beat off a *leeshore:* for the recesses of the Cardigan Bay are deep, and without shelter.

It may be conceived that a current, so generated, on the south-east coast of Ireland, (and possibly augmented by a portion of the stream from the Bay), would *shoot off* to the north-eastward, pointing towards the Bay of Cardigan; as it

cannot *turn short round* so acute a point, as that formed by the Cape of *Carnsore:* such being the nature of all currents, whether of water, or of air. And vessels will be carried to the north-eastward, accordingly, whilst they continue in the stream of the current. The *southerly* current which passes by Dublin, enters probably into, and merges in the stream in question; as the same kind of current, on the eastern side of England, falls into that, which passes the Strait of Dover, and afterwards runs along the Coasts of Flanders, Holland, &c.

The use of being well informed, concerning these *partial* currents, in narrow seas, is obvious: since the want of such information may, in a moment, be fatal to a ship, and her whole crew. This cannot be more strongly enforced, than by calling to mind the circumstances under which a frigate was lost, with her crew, during the war, just terminated. She sailed either from the Downs, or the Thames, to the *Helder*, and ran in the night, under full sail, on one of the shoals, lying before it. Had the Commander known that there is a general *set*, or current, from the strait of Dover (i. e. at the *back* of the Goodwin Sand), along the Coast of Flanders, Holland, Jutland, &c., and which is estimated to be equal to twenty-five miles, on an ordinary passage to the Texel, he would not have run on, during the night: or, at least, without the precaution of sounding.

Again, the Britannia India ship, in 1809, was lost on the *back* of the Goodwin Sand; probably through ignorance of the *acceleration* of the same current, during a violent gale at west, or south-west. A pilot would be reckoned deficient, who did not know the direction and force of the several streams of tide, at all seasons, within his province. There

is a current generally, if not constantly, running up the British Channel; that is, the eastern tides are the strongest; and in stormy weather from the west, *run longer* than the western, (or ebb) tides. At the same time the level of the channel is raised two feet or more, above that of the North Sea; and consequently, the former will discharge plentifully into the latter. Here then, is an *acceleration* of the current; and which men who have the charge of piloting of ships, ought to have known. And who can be ignorant of the high level of the Channel, when they know that the ports in the Channel are some feet deeper in strong westerly winds, than at ordinary times?

Although the following remarks do not apply to the *Scilly*, or *Thwart* current, yet as relating to currents, that at all times affect the navigation around the British Islands; it is hoped that the utility of inserting them, may atone for their being out of place.

It is proper to state, that the facts here set forth, are assumed, on the ground of *detached* notices, and not from a connected chain of observation. Indeed it could hardly have been expected. And it is also proper to be stated, that the currents here intended, only form a portion of the ordinary stream of tide, along the coast. And it is the *difference* between the degrees of velocity of the opposite streams, on the same coast, that constitutes the current in question: as indeed, it cannot be manifested, in any other way. An instance has just been given in the British Channel.

Where rivers form any quantity of alluvial ground, at their entrance into the sea, there, most infallibly, the direction of the

sea current will be shown, by the arrangement of the alluvial ground ; or by the sand or mud banks contiguous to it, in the sea. The process is explained in a note : and those, to whom I may not have rendered myself intelligible, may easily satisfy themselves, by observing the junction of any two streams, that are very much disproportioned to each other, in point of bulk ; and in which, no art has been employed to counteract the natural course of things.*

* The point of junction of two rivers (or of a river with the sea, provided that the sea has a predominant stream of current) will always form an *acute angle*, if the soil, through which they run, be not of a texture, firm enough to resist the corroding power of the stream ; but composed of alluvial matter, deposited by one, or both of the waters ; as is ordinarily the case. This point of junction, may be either *firm alluvial land*, or a *bank* of *sand or mud*, under water ; as the case may happen. And finally, the acute angle of junction will always point in the direction of the stream of the *recipient* water ; be it a river, or the sea.

The reason of this is, that two streams, at their confluence, have a natural tendency to *slide* into each other, as the easiest mode of effecting their junction ; and were they, either by reason of the natural solidity of their banks, or by artificial means, compelled to join at right angles, or at a very large angle, the meeting of their waters, in a case where they had any degree of rapidity, would produce an agitation ; that would prove injurious to their banks, and inconvenient to the navigation.

For the sake of illustration, let it be supposed that a small river is conducted artificially, into a larger one (or into a sea which has a current along shore,) through a *cut* made through the alluvial soil ; and the angle of junction to be very large, or approaching to a right angle ; and without any artificial aid, to keep it in that state ; the following train of consequences would ensue : (It is to be supposed, of course, that the recipient river had its bed previously enlarged, to receive the other, in order to prevent floods).

The first effect would be, that the head of the *adjunct* river, entering with an almost perpendicular course, into that of the *recipient*, would meet with so much resistance from it, that it would be partly beaten back, and compelled to seek its way along the bank of the recipient river. This bend in its course, would induce such a pressure on the bank, at the *lower* angle of junction, as would soon wear it away ; and an *oblique course* of approach, of the whole body of the adjunct stream,

In effect, the *embouchures* of rivers, situated in alluvial ground, always *point in the direction of the stream of the sea current*. The sand, mud, or gravel banks formed by the same current, *lie in the same direction;* and with their *narrowest*, or *sharpest* point, downwards.

In many places along the coast, water courses are found to terminate in long narrow lakes, with narrow sandy tracts, between them and the sea (through which the water still oozes, though all appears to be *stagnant*). These also have been formed by the sea currents, and point the same way. Originally, these water-courses, or rivers, gained the sea, near the place where the *head* of the lake now is; but the sea current, forming sandy alluvion, along that part of the coast, the river kept its course within it, along the former sea shore;

would commence. In the mean time, *a triangular space* of *still water*, would be formed between the *original* upper angle of junction, and the *new* one, occasioned by the obliquity of course: and this *still water*, as is its nature, would let drop the mud and sand which it had held suspended, whilst in motion; and thus begin to form *a triangular bank*, of the same shape and extent,*

Here then the operation is commenced, in all its parts: and the *triangular bank* by its being constantly on the increase, will force the adjunct stream to borrow still more and more, on its opposite bank; which will gradually wear away, until the *angle of junction* of the two waters, becomes so acute, that the adjunct stream no longer meets with any resistance, from that of the recipient; but may be said to *slide* into *contact* with it. This then, is the *natural state* of the junction of streams: but after all, the point of junction will, although almost imperceptibly, move downwards; because the *triangular bank* must continue to receive additions, if left to itself. Mean time, the body of it rises by the continual depositions, above the surface of the water, and becomes firm alluvial land; its *apex* being the point of junction of the two waters; and its *direction*, of course, the same with that of the stream of the recipient water.

When two streams that are nearly equal to each other, in respect of bulk, and of velocity, join; each of them, as it were, asserts *its own rights*; and the collective

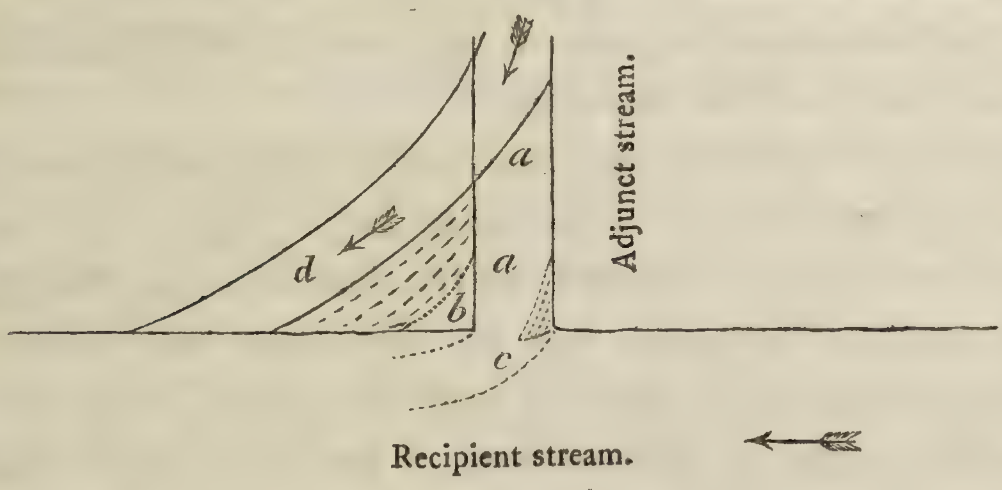
* The reader is referred, for explanation, to the sketch on the following page.

but growing sluggish, for want of *declivity*, it formed a lake within the increasing alluvion; and occasionally, in time of floods, forced open again, a communication with the sea. These lakes equally prove a sea current, as well as its line of direction.

If the reader requires examples, on a large scale, the mouths of the *Senegal* and *Missisippi* rivers, may be referred to. In both cases the currents are well known. And for lesser ones

stream takes a direction, which is generally a mean, between those of the two streams, whilst they remained in a separate state. And in like manner, the falling in of a stream that bears any proportion to the recipient river, will occasion a proportionate determination of the collective stream, towards the line of the former course of the adjunct.

It will be recollected, that all that was meant to be said here, applies to the courses of streams, through alluvial ground: and also to such, as have some degree of velocity.



(*a*) Artificial cut, through alluvial ground.

(*b*) The angle of the bank first worn away.

(*c*) The commencement of a triangular bank formed by the *still water* above the point of junction.

(*d*) Progress of the adjunct stream, towards a more permanent junction.

the rivers on the coasts of our own Islands; as those of *Christ-church, Shoreham, Newhaven, Orford, Yarmouth, Dublin,* &c. The small water-courses prove, equally with the large ones, the state of the current.

It is assumed, on the ground of various notices that the author has collected, that along the western side of Ireland, occasioned by the prevalence of westerly winds, the preponderance of the stream is northwards:* that it turns round the north end of the Island (or at least, a part of it does,) and thence southward, along the eastern coast: probably the whole way to Carnsore Point; but certainly past Dublin. The effect of a southerly current, or prevailing southerly tide, over the northern, is visible in the present *outfall* of the Liffey; and still more in the traces of the former one; previous to the erection of the *long wall:* both having an inclination to the south. And possibly, the *direction* of the wall has itself occasioned a part of the present difficulty, arising on the matter of the pilotage.

And here I beg leave once more to quote Captain Mead; who, speaking from his own experience, says (page 11), "on the " western Coast of Ireland, off the *Shellocks* (qu. Skelligs?) " the *northern indraught,* was not less than four leagues in " twenty-four hours, even in *moderate* gales. Also, off the " western coast of *Lewis* Island, I find it stronger than in " *soundings,* [that is, more than six or eight miles in the " twenty-four hours]; and also, off *Foul* Island, (Shetland) " something weaker than the latter."

Along the south-eastern coast, from the Mizen head, and

* Articles of various kinds, known to have come from the southward, and south-east, are continually casting up in Galway Bay.

Cape Clear, to Carnsore Point, the Author is less informed, in respect of notices from others, than concerning any other part of the Coast; but having visited it repeatedly, and considered all the circumstances belonging to it, he is firmly persuaded that there is a *north-easterly* current; and that it is *this* stream, prolonged from Carnsore Point, that carries vessels to the eastward of their course, in their way up the Irish Sea. (See above, page 194.)

The same kind of northerly stream, and occasioned, probably, by the same cause, is produced on the western coast of Scotland; from whence it turns round the north end of the island, and thence southward, along its eastern side, as far as Harwich; where it falls into the strait of Dover, or Channel Current, which comes up at the back of the Goodwin Sand.

The Channel Current has already been mentioned (page 196.) and can hardly be questioned, as to its existence, when the circumstances already set forth, are considered: such as the *elevation* of the *level* of the Channel, at times, by two feet or more; the *longer continuance* of the *eastern* stream of tide, than the *western;* together with the stream that runs to the north-east, from the Strait of Dover, along the whole Coast of Flanders, Holland, and Jutland: and which, affording, as is said, a help of twenty-five miles, ordinarily, between the Thames and the Texel, a run of only 160 miles, or less; cannot but be referred in part, to the Channel, or Dover Current. (For more particulars concerning the current at the Strait of Dover, &c. the reader is referred to Vol. XVIIth. New Series, of the Society's Transactions.)

At the mouth of the Baltic sea, the Jutland current is joined by the *outfall* of the former; which, at all times, receives more

water than it can evaporate. The collective stream then proceeds along the coast of Norway; continuing its northerly course. Off the *Naze*, it is known to run two miles *per* hour.

Such is the general course of the stream of current, around these Islands. There are, however, many particular exceptions to be made; as happens in the case of tides, where a deep recess in the coast often disturbs the uniformity of the general course of the stream.

CHART of the TRACKS of the HECTOR and ATLAS EAST INDIA SHIPS, in 1778 & 1787, Exhibited with a Design to prove the Existence of a CURRENT, between USHANT and IRELAND.
With Additions in 1815.

XII. *Some experiments on a solid compound of iodine and oxygene, and on its chemical agencies. By Sir* HUMPHRY DAVY, *LL.D. F. R. S.*

Read April 20, 1815.

In the two papers containing researches on iodine which the Royal Society has done me the honour of publishing in the Transactions, I have described a class of bodies consisting of iodine, oxygene, and different bases analogous to the hyperoxymuriates. In the last of these papers, I mentioned, that I had not been able to procure any binary combination of iodine and oxygene from these compounds, neither by the method proposed by M. GAY LUSSAC, namely, the action of sulphuric acid on the oxyiode of barium, nor by other methods of my own institution; and that in experiments on the effects of the acids on the oxyiodes, new combinations only were formed. I have lately resumed this enquiry, and by pursuing a new and entirely different plan of operation, I have at last succeeded in combining oxygene and iodine. In the following pages I shall describe the circumstances which led me to ascertain the existence of this compound, and I shall detail some experiments on its analysis and its chemical agencies.

In the course of my researches, I observed, that when a solution of the compound of iodine and chlorine was poured into alkaline solutions, or even into certain muriatic solutions, the precipitate was an oxyiode; and this fact seemed to indicate, that iodine had a stronger attraction for oxygene than

chlorine; iodine, likewise, has an attraction for chlorine; it appeared, therefore, extremely probable, that euchlorine, or the gaseous combination of oxygene and chlorine, would be decomposed by heat, and two compounds formed, one of oxygene and iodine, and the other of iodine and chlorine, or that a triple compound would be produced from which chlorine could be easily separated, and on submitting the idea to the test of experiment, I found that I had not been deceived.

To produce the compound of oxygene and iodine, it is necessary merely to bring the euchlorine and iodine together at the ordinary temperature of the atmosphere. As soon as the euchlorine comes in contact with the iodine, there is an immediate action, its colour changes to bright orange, and a liquid is formed. When the euchlorine is in sufficient quantity, a white substance likewise appears. By the application of a gentle heat, the orange compound of chlorine and iodine may be made to rise in vapour; and the compound of oxygene and iodine remains.

When this compound is required to be dry, the euchlorine should be passed through dry muriate of lime (calcane) before it is admitted to the iodine. The apparatus that I have employed for producing the substance is a curved bent tube, in the form of an inverted L (ꓶ), closed at one end, the closed leg of the tube being longest, and which serves as a retort for generating the gas; a thin long-necked glass receiver for containing the iodine, and a curved tube of smaller diameter than the first, and cemented or ground into it for conveying the gas into the receiver. The muriate of lime is placed in some dry paper in the upper part of the large curved tube;

and to produce the substance from 40 grains of iodine, 100 grains of the hyperoxymuriate should be used, and four times the quantity of solution of muriatic acid of specific gravity about 1.105; a very small spirit lamp should be employed to generate the gas; and to prevent explosions, the heat should be applied with great care, and only to the bottom of the tube.

The compound of oxygene and iodine when entirely freed by heat from the compound of oxygene and chlorine, appears as a white semi-transparent solid; it has no smell, but a strong astringent sour taste. Its specific gravity is considerable, for it rapidly sinks in sulphuric acid. When heated strongly, it decomposes, undergoing fusion at the moment, and is entirely converted into gaseous matter and iodine, leaving no residuum whatever.

It requires for its entire decomposition, a heat which is rather below the boiling point of olive oil, and there seems to be little or no increase of temperature in the process.

Its nature is proved both by analysis and synthesis, for when euchlorine acts upon iodine, the volatile substance produced, has all the characters of the body produced by the immediate action of chlorine on iodine; and when the compound I am describing is decomposed in a pneumatic apparatus, the gas formed is found to be pure oxygene, and the solid sublimate produced, is pure iodine.

I endeavoured to determine the proportions of the elements in the compound, by decomposing it in glass tubes carefully weighed, and ascertaining the loss of weight of the tube, and the volume of oxygene evolved. I have used very small quantities of the substance, but as my balance is delicate, I

do not think there can be any considerable error in the results. I give those which I consider as the most accurate.

In one experiment, 3 grains of the substance afforded a quantity of oxygene equal to 517.3 grain measures of water, and lost in weight .68. In a second experiment, 2 grains afforded 348.3 grain measures of oxygene. In a third experiment, 1 grain yielded 191 grain measures of oxygene.

Many experiments that I have lately made, have convinced me, that in my first paper I rated the number representing the proportion in which iodine combines too low; indeed at the time, I stated that my results afforded rude approximations, they demonstrated merely that iodine was represented by a very high number. In an experiment recently made with care, 50 grains of the iode of potassium decomposed by nitric acid, afforded 32.8 grains of nitre. According to this result, the number representing the proportion in which iodine combines is 227.3; but I do not venture to state this number as exact, as I am not secure of the purity of the hydrate of potassa from which the iode was made.

The compound of iodine and oxygene is very soluble in water; it slowly deliquesces in a moist atmosphere, but remains unaltered when the air is dry; its solution first reddens, and then destroys vegetable blues, and reduces other vegetable colours to a dull yellow. When its aqueous solution is heated, as the water rises in vapour, it gradually thickens, gains the consistence of a syrup, becomes pasty, and at length by a stronger heat yields the solid substance unaltered; unless a sufficient heat is applied to decompose a portion of it, when it gains a purplish tint apparently from some iodine set free. The pasty substance that it forms is evidently an hydrate, for it yields moisture during its decomposition.

Its action upon inflammable bodies is such as might be expected from its composition. When it is heated, mixed with charcoal, sulphur, resin, sugar, or the combustible metals in a finely divided state, detonations are produced; and its solution rapidly corrodes all the metals which I have exposed it to, and it acts both upon gold and platinum, but much more intensely on the first of these metals.

When a solution of it is poured into solutions of the alkalies or alkaline earths, or when made to act on their carbonates, oxyiodes or triple compounds of oxygene, iodine, and the metallic bases, are the results. By its action on solution of ammonia, a substance is produced apparently the same as that which is formed by the action of the compound of iodine and chlorine, saturated with chlorine on the same solution, and which I have mentioned in a former Paper, and which, consequently, must be regarded as an oxyiode of ammonia.

When an aqueous solution of the compound is poured into a solution of the soluble salts of baryta and strontia, a copious precipitate of their respective oxyiodes is produced. The oxyiode of barium, as I have mentioned in my last communication on iodine, is a compound very slightly soluble in water, that of strontium is rather more soluble; and those of calcium, magnesium, glucinum, ittrium, aluminum, zirconum, are more so, and I believe in the order in which they have been named.

It forms combinations with all the metallic oxides I have tried its agency upon, and precipitates lead and mercury from their nitrous solutions.

The action of the compound upon acids is much more singular than that upon alkalies, earths, or metallic oxides. It

appears to form combinations with all the fluid or solid acids to which I have been able to expose it, that it does not decompose. When sulphuric acid is dropped into a concentrated solution of it in hot water, a solid substance is precipitated, which consists of the acid and the compound; for on evaporating the solution by a gentle heat, nothing rises but water. On increasing the heat in an experiment of this kind, the solid substance formed, fused; and on cooling the mixture, rhomboidal crystals formed of a pale yellow colour, which were very fusible, and which did not change at the heat at which the compound of oxygene and iodine decomposes, but sublimed unaltered. When urged by a much stronger heat, it partially sublimed, and partially decomposed, affording oxygene, iodine, and sulphuric acid.

With hydro-phosphoric acid, the compound presents phenomena precisely similar, and they form together a solid, yellow, crystalline combination.

It dissolves in solution of hydro-phosphorous acid, but on heating the combination, iodine is immediately produced by a decomposition of part of the compound, and the remaining part unites to the phosphoric acid formed.

When hydro-nitric acid is poured into a concentrated solution of it, white crystals form in plates of a rhomboidal figure, and which when dried, partially decompose, and partially sublime at a much lower heat than the sulphuric or phosphoric compounds, and afford hydro-nitric acid, oxygene, and iodine.

It dissolves in solution of oxalic acid, but by a very gentle heat, the oxygene of the compound acts on the inflammable bases of the acids, and iodine and carbonic acid are disengaged in great quantities.

By liquid muriatic acid, the substance is immediately decomposed, and the compound of chlorine and iodine is formed.

When boracic acid was added to a solution of the new compound, it dissolved in it by heat, and did not crystallize on cooling. By evaporation, a solid white substance was procured, not so easily decomposed by heat as the compound itself.

The taste of all these acid compounds is very sour, though in different degrees of intensity: they redden vegetable blues, and they dissolve gold and platinum. When they are made to act on the alkalies or earths, or on saline solutions which they are capable of decomposing, common neutral salts and oxyiodes are formed at the same time.

The facts of the combination of the new compound with acids, serve to explain the phenomena of the action of these substances, on the oxyiodes which I have described in my last paper on iodine, and they confirm the opinions there stated on the nature of this action. The substance procured by M. Gay Lussac, by the action of sulphuric acid on the solution of the oxyiode of barium, and which he has supposed to be a pure combination of oxygene and iodine mixed with a little sulphuric acid, has evidently for its base the combination just now described of sulphuric acid and the new compound, and, as I have shown, it likewise contains baryta. However minute the quantity of sulphuric acid made to act on oxyiode of barium, a part of it is always employed to form the compound acid; and the residual fluid contains both the compound acid, and a certain quantity of the original salt.

That this compound acid is a true chemical combination, is evident from the observations already detailed, and from

its crystalline form. There is every reason to believe, that the proportions of its elements are definite. In one experiment I found, that a small quantity of the new compound in being converted into the rhomboidal crystals, gained rather less than half its original weight from the addition of the acid, i. e. 2 grains became 2.8 grains.

In experiments in which the products of the decomposition of the compounds from phosphoric and sulphuric acids were collected, the acids disengaged were found in their state of hydrates, from which it is probable, that the crystalline compounds are hydrates, and that the common acids carry their definite proportion of water into the combination. It is not indeed unlikely, that the presence of water is connected with the phenomenon of combination, and there is an instance of this kind which I long ago pointed out. Sulphurous acid gas, and nitrous acid gas, appear to have no action on each other, unless water be present; but with the vapour of water they form a solid crystalline hydrate.

Reasoning from analogy, it is probable, that a compound of oxygene may be formed, containing less oxygene than the new compound. I have made many experiments with the hope of discovering a body of this kind; but without any decided success. When the solution of the new compound is made to act on the double compound of iodine and the alkaline metals, iodine is produced, which during its sublimation, yields no gaseous product. Iodine heated in a solution of the new compound slightly colours it, but this appears to be merely in consequence of its combining with the water; and the iodine rises in vapour with the water without decomposing the compound. In some experiments on the action of

euchlorine on iodine, in which the iodine was in great excess, the solid substance formed had a chocolate tint; but this may possibly have depended upon a small quantity of free iodine, and when dissolved in water, it afforded by the evaporation of the water, the white compound only.

I detailed in my last paper on iodine, some unsuccessful attempts to procure a compound of oxygene and iodine from the chlorionic acid, the substance produced by the agency of the combination of iodine and chlorine in water, on the idea that water was decomposed in this experiment. I have made some further researches, on the supposition that it might contain a compound of iodine containing less oxygene than this new substance; but without any success: neither by distillation at very low temperatures, nor by the action of small quantities of oxide of silver, nor by any other means, have I been able to separate any compound of oxygene from it; and when it forms triple compounds, the oxyiodes, by its action upon alkalies, or earths, or metallic solutions, it appears that the oxygene of the alkalies or earths is only newly combined at the moment of its operation upon them, an effect assisted by the attraction of the bases of the earths for chlorine. The conclusion which I formed, that the chlorionic acid is a simple combination of the chlorionic sublimate in water, is still farther proved by the circumstances of the action of muriatic acid on the new solid compound of oxygene and iodine.* Page 209.

* The chlorionic acid offers an easy method of procuring pure baryta. By dropping a solution of it into solution of muriate of barium, as I have shown in my last paper on iodine, a precipitate of oxyiode of barium is produced, which when decomposed by a strong heat, yields pure baryta, the attraction of oxygene for barium being, as I have stated, stronger at this temperature than that of iodine.

As I have called the compounds of oxygene, iodine, and bases, *oxyiodes*, I venture to propose a name in conformity, that of *oxyiodine* for the new solid compound, and *oxyiodic* acid, for the acid compound it forms with water. M. GAY LUSSAC, as I am informed, has proposed in a paper which I have not yet been so fortunate as to procure, but which is said to contain many new and important facts, the name of iodic acid for the compound of oxygene and iodine, the existence of which he conceived he had proved by his experiment on the action of sulphuric acid on the oxyiode of barium, and the terms iodats for the substances, consisting of oxygene, iodine, and bases. I am willing to pay every compliment to the sagacity of this ingenious chemist, in anticipating the knowledge of the nature of a body, the separate existence of which he had not demonstrated by experiment; but the term *iodic* acid does not appear to me sufficiently definite. For the hydroionic acid, and the chlorionic acid, as well as the oxyiodic acid, may be all called as a class iodic acids, or acids from iodine, and the termination in *at*, would place the oxyiodes in the common class of neutral salts, from which they differ in many respects. When they become binary compounds in consequence of their decomposition by heat, though they lose all their oxygene, their neutral and saline character remains unaltered, which is not the case with any other known class of bodies, except the hyperoxymuriates; and the terms iodes and oxyiodes which I proposed in the first paper, in which the distinction between these bodies was pointed out, sufficiently express the nature of the double and triple compound, and the difference between them.

I am desirous of marking the *acid* character of oxyiodine

combined with water, without applying the name acid to the anhydrous solid. It is not at all improbable that the action of the hydrogene in the combined water is connected with the acid properties of the compound; for this acid may be regarded as a triple combination of iodine, hydrogene, and oxygene, an oxyiode of hydrogene, and it is possible that the hydrogene may act the same part in giving character, that potassium, sodium, or the metallic bases perform in the oxyiodes; and as hydrogene combined with iodine forms a very strong acid, and as this acid would remain, supposing all the oxygene to be taken away from the oxyiodic acid, it is a fair supposition that its elements must have an influence in producing the acidity of the substance.

Rome, February 10, 1815.

XIII. *On the action of acids on the salts usually called hyperoxymuriates, and on the gases produced from them.* *By Sir* Humphry Davy, *LL.D. F. R. S.*

Read May 4, 1815.

The effects produced when concentrated hydro-sulphuric acid (oil of vitriol) is poured upon hyperoxymuriate of potassa, have been often objects of chemical discussion; the acid and the salt, it is well known, become deep orange, and if any moisture is present, or if heat is applied to the mixture, a detonation occurs. In a paper read before the Royal Society, I have ventured to suppose, that these phenomena depend upon the developement and sudden decomposition of the compound of chlorine and oxygene, which I have named euchlorine.

A statement, which I understand has been made by M. Gay Lussac, namely, that a peculiar acid, which he has called chloric acid, may be procured from the hyperoxymuriate of baryta by sulphuric acid, led me to examine the action of acids on the hyperoxymuriates under new circumstances, and I have made some observations which appear to me not unworthy of being communicated to the Royal Society.

If 30 or 40 parts of sulphuric acid be poured upon one part of dry hyperoxymuriate of potassa in a wine glass, and the salt be agitated in the acid, there is a very slight effervescence only, the acid becomes of a deep orange tint, and white

fumes, mixed with orange fumes, fill the upper part of the glass, which have a very peculiar and not a disagreeable smell.

The *slight* effervescence taking place in this process, induced me to suppose that the substance which coloured the acid must contain a larger proportion of oxygene than euchlorine, for I have shown, in a work published in 1812,* that hyperoxymuriate of potassa contains six proportions of oxygene; and by its decomposition 2.5 volumes of oxygene ought to be evolved for every volume of chlorine; and euchlorine procured from the hyperoxymuriate of potassa by solution of muriatic acid, yields only one volume of oxygene, and two volumes of chlorine.

I endeavoured to procure the substance which coloured the sulphuric acid during its action upon hyperoxymuriate of potassa, and after several failures, in which explosions took place, I at length succeeded in the following manner. Dry oxymuriate of potassa in powder was mixed with a small quantity of sulphuric acid, and they were rubbed together with a spatula of platinum till they had incorporated, and formed a solid mass of a bright orange colour. This mass was introduced into a small retort of glass, and exposed to the heat of water which was gradually warmed; a bright yellowish green, elastic fluid arose from the mixture, which was rapidly absorbed by water, giving to it its own tint, but which had no sensible action on mercury.

To make this experiment without danger, not more than 50 or 60 grains of the hyperoxymuriate should be employed, great care should be taken to prevent any combustible matter from being present, and the water should not be permitted to

* Elem. of Chem. Phil.

attain a temperature equal to 212°, which may be easily managed by mixing it with alcohol. There are dense white fumes when the mixture is first made, but there seems to be no heat produced; a small quantity of the orange gas is disengaged at this time; but the greater part of it remains attached to the sulphuric acid in the solid mass, and is expelled from it by the heat.

The gas procured by this process over mercury, when compared with the gas procured from the hyperoxymuriate, by liquid muriatic acid, is found to have a much more brilliant colour, is much more rapidly absorbed by water, has a peculiar and much more aromatic smell, unmixed with any smell of chlorine. It destroys moist vegetable blues without previously reddening them. When it is heated to a temperature about that of boiling water, it explodes with more violence than euchlorine, and greater expansion of volume, producing much light. After the explosion over mercury, rather less than three (from 2.7 to 2.9.) volumes appear for two of the gas decomposed, and of these, two are oxygene, and the remainder chlorine.

A little chlorine is always absorbed by the mercury during the explosion of the gas; and it appears reasonable to conclude, that the deep yellow gas is in reality composed of two in volume of oxygene, and one of chlorine, condensed into the space of two volumes, and that it consists in weight, of one proportion of chlorine 67, and four of oxygene 60.

None of the combustible bodies which I have tried, decompose this gas at common temperatures, except phosphorus; this when introduced into it, occasions an explosion, and burns in the liberated gases with great brilliancy.

Its saturated solution in water is of a deep yellow colour,

it does not taste sour, but is extremely astringent and corroding; when applied to the tongue, it leaves for a long while a very disagreeable sensation.

It occurred to me that the gas procured from the hyper-oxymuriate by the action of liquid muriatic acid, might be a mere mixture of this gas and chlorine; and two in volume of this gas and three in volume of chlorine, would produce by explosion the same products as euchlorine. The only fact which I am acquainted with, opposed to the idea, is the circumstance of dutch foil not burning spontaneously in the gas from muriatic acid, which might be expected if it contained as much as $\frac{3}{5}$ of uncombined chlorine; though the force of this argument is suspended, till it is supported by an experiment showing that dutch foil inflames in a mixture of two of the deep yellow gas, and three of chlorine. I have not yet been able to procure at Rome, metallic foil fitted for this experiment.

I have ascertained that the gas from hyper-oxymuriate and muriatic acid, though it acts much more slowly upon water than the other gas, yet in the end gives it the same tint and properties; and when much of it is exposed to a small quantity of water, it always leaves a residuum of chlorine, so that if it be not a mixture, but a compound, the new gas is formed from it by the action of water.

The action of hydro-nitric acid on the hyper-oxymuriate, affords the same gas as that produced by the action of sulphuric acid, and a much larger quantity of nitric acid may be safely made to act on the salt; but as the gas must be procured by solution of the salt, it is always mixed with about $\frac{1}{5}$ of oxygene. From the solid mixture made with sulphuric acid, I have obtained a gas containing only $\frac{1}{20}$ of oxygene; the fifth

proportion obtained in the experiments with nitric acid, being evolved during the time the mixtures were made.

The saturated solution of the gas affords white fumes, similar to those produced at the moment the hydro-sulphuric mixture is made, from which it is probable, that these fumes consist of a hydrate of the gas.

The saturated solution, when mixed with solution of fixed alkalies, or of ammonia, does not immediately lose its colour, nor neutralise the alkalies; but after some time the effect is produced, and hyper-oxymuriates are obtained, (probably mixed with a minute quantity of muriates). The solution exposed to air, or suffered to remain in close vessels, becomes soon colourless; and I am inclined to believe that this depends upon a decomposition of water, for some of it exposed to a small quantity of air rather increased its volume.

I shall not propose to give any name to this substance, till it is determined whether euchlorine is a mixture or a definite compound, and I hope soon to have the means of making a decisive experiment on this subject.

It appears that this new substance, though it contains four proportions of oxygene, is not an acid; and hence it is probable, that the acid fluid compound of oxygene, chlorine, and water, which M. Gay Lussac calls chloric acid, owes its acid powers to combined hydrogene, and that it is analogous to the other hyper-oxymuriates, which are triple compounds of inflammable bases, chlorine, and oxygene, in which the base and the chlorine determine the character of the compound. Muriate of potassa, (potassane) is a perfectly neutral body; and when six proportions of oxygene are added to it, it still remains neutral. Muriatic acid (chlorine and hydro-

gene) is a strong acid; and according to the relation above stated, it ought not to lose its acid powers by the addition of six proportions of oxygene. Till a pure combination of chlorine and oxygene is obtained, possessed of acid properties, we have no right to say that chlorine is capable of being acidified by oxygene, and that an acid compound exists in the hyper-oxymuriates. We know that chlorine is capable of being converted into an acid by hydrogene, and, as I mentioned in my last paper, where this principle exists its energies ought not to be overlooked; and all the new facts confirm an opinion which I have more than once before submitted to the consideration of the Society, namely, that acidity does not depend upon any *peculiar* elementary substance, but upon *peculiar combinations* of various substances.

Rome, Feb. 15, 1815.

NOTE.

SINCE my return to England, I have made some farther investigations on oxyiodine, on the oxyiodes, and on the deep yellow gas. The portable apparatus which I employed in Italy, enabled me to operate only on very minute quantities of oxyiodine; I have lately made my experiments on a larger scale.

Thirteen grains of oxyiodine decomposed by heat, afforded 9.25 cubical inches of oxygene: and 48 grains of oxypotassame or oxyiode of potassium, yielded when decomposed by heat, 31 cubical inches of oxygene gas: and 30 g ains of potassame or iode of potassium (a portion of the salt so decomposed,) afforded by treatment with nitric acid 17.8 grains of dry nitre. These results give the number 246 as the number representing iodine, and prove that oxyiodine consists of one proportion of iodine and five of oxygene; and that the oxyiodes contain six proportions of oxygene

The deep yellow gas when mixed with chlorine in the proportion of 2 to 3, or even of 2 to 2, deprives it of the power of acting upon Dutch foil, though 1 of chlorine when mixed with 2 of common air, still burns this substance. Hence it appears probable, that the deep coloured gas and chlorine have a chemical action on each other, and that euchlorine is not a simple mixture of them. I hope soon to be able to present to the Society, some new results on this subject.

London, June 12, 1815.

XIV. *Farther analytical experiments relative to the constitution of the prussic, of the ferruretted chyazic, and of the sulphuretted chyazic acids; and to that of their salts; together with the application of the atomic theory to the analyses of those bodies. By* Robert Porrett, *jun. Esq. Communicated by* W. H. Wollaston, *M. D. Sec. R. S.*

Read May 11, 1815.

The Royal Society did me the honour of printing in the volume of their Transactions for last year, a paper of mine on the nature of the salts termed triple prussiates, and on acids formed by the union of certain bodies with the elements of the prussic acid.

In that paper, I endeavoured to prove, that the elements of the prussic acid would combine with a certain proportion of black oxide of iron, and form a peculiar, and hitherto unknown acid, for which I proposed the name of the ferruretted chyazic acid. I showed that this was the real acid portion of the salts which had received the erroneous appellation of triple prussiates, and that the property of combining with the prussic acid, so as to change its nature, and increase its acid properties, was not confined to the black oxide of iron, but was possessed probably by many other bodies, but certainly by sulphur, which formed with it another acid, for which I proposed the name of the sulphuretted chyazic acid. The paper also contained some analytical researches into the proportions

in which the elements of these new acids are combined in them, and also into the proportions in which they unite with different saline bases.

My object in this paper, is to add to the analyses contained in the former, two analyses which I have since made; and then to apply to the whole, the admirable theory of Dalton, by which the proportions in which bodies can combine, are conceived to be governed by the relative weights of their chemical atoms, and also Berzelius's addition to this theory, by which the combinations of oxides with one another, are conceived to take place in such a manner, that the oxygen contained in one of these bodies, is either equal to, or is a multiple by a whole number, of the oxygen contained in the others.

I begin with describing the two analyses to which I have just alluded.

Analysis of prussiate of mercury.

A. Fifty grains of this salt finely pulverised, were kept at the temperature of 212° for six hours, at the end of which time, they weighed exactly the same as before.

B. Forty grains of this salt were dissolved in water and decomposed by hydro-sulphuret of potash: the products of this decomposition were prussiate of potash and black sulphuret of mercury; the quantity of the former could not be ascertained with accuracy, owing to the escape of much of the prussic acid, but that of the sulphuret amounted to 37.2 grains.

C. Disappointed in my attempt to estimate the quantity of prussic acid by the last experiment, owing to its very volatile nature, I availed myself of the property I had discovered

in the hydroguretted sulphurets, of converting the prussic acid at the moment they detach it from prussiate of mercury, into sulphuretted chyazic acid; which being much less volatile, and having a stronger attraction for alkaline bases than the prussic, could not escape from the liquid, and would give me the quantity of prussic acid it represented, by deducting from its weight, that of the sulphur which I knew to exist in it. I therefore dissolved 10 grains of prussiate of mercury in hot water, and poured hydroguretted sulphuret of soda into the solution until it no longer occasioned a black precipitate. This black precipitate when dry, weighed 9.3 grains; to the liquid from which it was separated, I added a few drops of diluted sulphuric acid; these caused a separation of a minute quantity of sulphur, which was got rid of by subsidence, after which I poured into it an aqueous solution of the two sulphates of copper, and of black oxide of iron, in which the former salt was to the latter by weight, as 2 is to 3, until no farther effect was produced. By these means I threw down the whole of the sulphuretted chyazic acid contained in the liquid, and collected it combined with protoxide of copper, in the form of an insoluble white salt, which weighed 9.7 grains.

But as 100 grains of this salt contain 40.62 grains of sulphuretted chyazic acid, composed of 26.39 sulphur and 14.23 prussic acid, according to my analysis, Phil. Tran. for 1814, page 549, Exp. C, (corrected by calculations in the Table facing page 230 of the present paper), therefore the before mentioned 9.7 grains represent 1.38 of prussic acid, which according to this experiment is the quantity existing in 10 grains of prussiate of mercury.

D. I had next to ascertain how much red oxide of mercury was represented by the 37.2 grains of black sulphuret obtained in Experiment B, and by the 9.3 grains of the same substance obtained in Experiment C. In order to effect this, I made the following experiment: 25 grains of corrosive sublimate were dissolved in water, and decomposed by hydro-sulphuret of potash; the black sulphuret thus formed, weighed 21.5 grains, which, therefore, represents 19.94 grains of red oxide of mercury, that being the quantity contained in 25 grains of corrosive sublimate.

Then as 21.5 : 19.94 : : 37.2 : 34.48 the quantity of red oxide in 40 prussiate of mercury,

And as 21.5 : 19.94 : : 9.3 : 8.62 the quantity of ditto in 10 of ditto,

100 grains of prussiate of mercury are therefore composed of

Prussic acid, Experiment C. - -	13.8
Red oxide of mercury, Experiment B. C. and D.	86.2
	100.0

Analysis of prussic acid.

Being very desirous of accomplishing the analysis of this acid if possible, I considered very attentively the nature of the difficulties to be surmounted in order to effect it. The principal ones appeared to me to be the following.

1st. That of always ascertaining with precision, the quantity which is the subject of analysis.

2d. That of effecting its combustion with oxygen in such a manner, that while, on the one hand, the whole of its carbon and hydrogen should be oxygenated, so on the other, that none of its azote should undergo this process.

3d. That of determining with great accuracy, the quantity of oxygen which combines with the elements of the prussic acid during its combustion, so as after allowing for what has been expended in the formation of carbonic acid, to be able to infer with confidence, from the disappearance of the rest, the quantity of hydrogen which was contained in the acid.

The property which the prussic acid possesses of assuming the liquid form at a low temperature, and that of a gas or vapour at common temperatures, the volume of which is materially influenced by mixture with other gases, and by slight alterations of temperature and pressure; did not appear to me to be favourable to the employment of it in an uncombined form for the purpose of its analysis.

I therefore determined upon employing it in the state of condensation in which it exists in prussiate of mercury, and this determination made me undertake the analysis I have just described of that salt; of the correctness of which, having satisfied myself, I conceived that I had overcome the first difficulty.

The second and third difficulties I thought would be best surmounted by employing, for the combustion of the prussic acid, the same oxide with which it is united in the prussiate of mercury, namely, the red oxide of that metal; increasing the quantity of it by multiples of that which the salt contains, until I found that the whole of the prussic acid was decomposed.

I made a number of experiments upon this plan, the results of which proved to me that the quantities of carbonic acid and of azote gases produced, did not arrive at the maximum until five times the quantity of red oxide of mercury contained

in the prussiate had been added to it, making together six of that oxide, to one of prussic acid, and that whenever a less quantity of the oxide than this had been employed, there always existed in the gaseous products, a portion of undecomposed prussic acid. I farther observed, that in all cases the volume of azote gas obtained, was exactly equal to that of the prussic acid decomposed, that the volume of carbonic acid gas produced was invariably twice that of the azote gas liberated in the same experiment, and that the carbonic acid produced accounted for only one third of the oxygen consumed. The observance of these laws by which the decomposition was regulated, enabled me in constructing the following Table (facing page 228,) to correct the minute and unavoidable inaccuracies of experiment, by the superior accuracy to be acquired by applying to the results so obtained, the corrections necessary to make them correspond with the abovementioned laws. It enabled me also to represent in the column denoting the measures of prussic acid gas, equal quantities by equal bulks; which, for the reasons before stated, experiment does not exactly show, and thus to render evident the true progress of its decomposition.

It may be proper before proceeding farther, to describe my mode of operating, in conducting the experiments from which the Table was compiled. This mode is similar in principle to that invented by Gay Lussac and Thenard in their Analysis of Animal and Vegetable substances, and improved by Berzelius. I am greatly indebted to these two French chemists, for the valuable information respecting this kind of analysis, which I have obtained from their Recherches Physico-Chymiques, and to Dr. Berzelius for that which I have received

from his Paper on the definite proportions in which the elements of organic nature are combined, published in Dr. Thomson's Annals of Philosophy for December last. It is to this information that I principally attribute the success which has attended the experiments of a similar nature, which I have made.

The method pursued by me, however, differs in several respects from that of either of the chemists just mentioned,

1st. In the apparatus employed, which is much more simple in my process

2dly. In the nature of the oxygenised body employed to effect the combustion.

3dly. In the method to which I had recourse, for proportioning the oxygenised to the combustible body, by making the former a multiple of that which enters into chemical union with the latter.

4thly. In decomposing a much less quantity of the combustible body at a time, than either of the above chemists.

In the present case, each of these alterations appeared to me to possess very decided advantages over the other methods. How far they may be applicable to other cases, I do not pretend to determine.

Having thus generally stated in what my process differed from former ones, I proceed to rather a more particular description of it.

I prepare the peroxide of mercury which I employ, by decomposing with pure soda, a solution of corrosive sublimate. Having weighed out the proportions of prussiate of mercury, and of the peroxide which I intend to decompose, I triturate them together in a small polished mortar of porphyry or agate

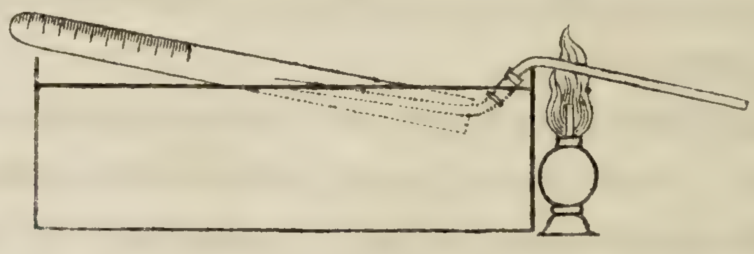
for several minutes, then collect into the centre of the mortar, what adheres to its sides, and repeat this alternate trituration and collection at least six times.

I then take a tube of glass $4\frac{1}{2}$ inches long, about the size of a common writing quill and tolerably stout, I close one end of it, and bend the other round, so that nearly an inch of that end forms a right angle with the rest. I call this the retort tube. I make a second tube similar to the first, except that instead of being closed at one end, it is open at both. I call this the adapting tube.

The retort tube is then charged with the mixed materials, by means of a small paper funnel, fixed with sealing wax to the top of the tube; the charge is introduced in about three equal portions, each of which is separated from the others by the introduction of a little coarsely powdered green glass; the charge generally occupies about $2\frac{1}{2}$ inches of the tube. After its introduction, the wax which fastened the funnel is softened by heat, and the funnel detached.

A graduated glass tube, capable of containing about $2\frac{1}{2}$ cubic inches, was next filled with mercury, and placed in the mercurial pneumatic trough, not in the usual perpendicular position, but with its upper end raised, but very little, from the horizontal situation, being about an inch above the surface of the mercury, while its lower or open end just dipped below that surface. In this position, the long leg of the adapting tube was passed up into it, which being open at both ends became filled with mercury; the short end of this tube was then connected with the short end of the retort tube, by means of a caoutchouc tube firmly tied to both. The long end of the retort tube when thus disposed, hung over the outside of

the end of the mercurial trough, in a position declining a little from the horizontal one towards the table. The decomposition was then commenced, by applying the flame of a spirit lamp to the empty part of the tube, and bringing it down gradually, so as to explode in succession the three strata of the mixture. The arrangement of the apparatus at the commencement of the process, will be instantly seen by an inspection of the annexed sketch.



When the retort tube was cold, it was separated, under the surface of the mercury, from the adapting tube, in such a manner, that any gas remaining in the latter might pass up into the graduated tube; the volume of gas collected, was then ascertained, making the necessary corrections for temperature, pressure, and the capacity of the retort tube, after which a solution of pure potash was passed up into it, and the diminution of volume which it occasioned was noticed; from the gas which remained, a deduction was made, for the quantity of atmospheric air in the upper part of the retort tube before the combustion, and which seldom exceeded $\frac{1}{30}$ of a cubic inch; the residual gas was considered as azote, and found to be so by all the tests to which I subjected it. The small quantity of solution of potash employed to effect the absorption was then examined, and if, besides carbonic acid, it was found to contain prussic acid, I concluded that I had not employed enough of the red oxide of mercury in the

TABLE showing the results of the decomposition by heat of prussiate of mercury, by itself, and also when mixed with multiples by whole numbers of its base.

Materials before decomposition.			Products after decomposition.													
Prussiate of mercury and red oxide of mercury.	Weight of oxygene in oxide of mercury.	Total weight of materials.	Gases. Prussic acid.		Gases. Carbonic acid.		Gases. Azote.		Gases. Total.		Water.	Mercury.	Weight of oxygen in products. In the carbonic acid.	Weight of oxygen in products. In the water.	Weight of oxygen in products. Total.	Total weight of products.
	Grs.	Grs.	C. In.	Grs.	C. I.	Grs.	C. I.	Grs.	C. I.	Grs.	Grs.	Grs.	Grs.	Grs.	Grs.	Grs.
Grs. Prussic acid. Red. ox. merc.																
2.5 of prussiate of mercury or 0.344 + 2.155 Red oxy. merc.	0.159	2.5	0.395	0.2866	0.158	* 0.0732	0.079	† 0.0234	0.632	0.3832	0.1205	1.995	0.053	0.106	0.159	2.4987
Ditto, with 2.155 - or 0.344 + 4.31	0.319	4.655	0.316	0.2293	0.316	0.1463	0.158	0.0467	0.790	0.4223	0.2410	3.991	0.106	0.213	0.319	4.6543
Ditto, with 4.31 - or 0.344 + 6.465	0.479	6.81	0.237	0.1719	0.474	0.2195	0.237	0.0701	0.948	0.4615	0.3615	5.986	0.160	0.319	0.479	6.809
Ditto, with 6.465 - or 0.344 + 8.62	0.638	8.965	0.158	0.1146	0.632	0.2926	0.316	0.0934	1.106	0.5006	0.4820	7.982	0.213	0.425	0.638	8.9646
Ditto, with 8.62 - or 0.344 + 10.775	0.798	11.12	0.079	0.0573	0.790	0.3658	0.395	0.1168	1.264	0.5399	0.6025	9.977	0.266	0.532	0.798	11.1194
Ditto, with 10.775 - or 0.344 + 12.93	0.957	13.275	0.000	0.0000	0.948	0.4389	0.474	0.1401	1.422	0.5790	0.7230	11.972	0.319	0.638	0.957	13.274

* Weight of carbonic acid taken as 0.463 grain for a cubic inch.

† Weight of azote taken as 0.2956 grain for ditto.

combustion, and repeated the experiment with an increased proportion of it.

Such was my method of effecting the analysis of the prussic acid, and by which as will be seen in the last line of the Table, I succeeded in discovering that 0.3442 gr. of it were composed as follows:

Carbon	= to that in 0.4389 gr. of carbonic acid, or	0.1198
Azote	= to the weight of the azote gas collected	0.1401
Hydrogen	= to that in 0.7230 gr. of water -	0.0843
		0.3442

consequently that 100 grains contain

Carbon - -	34.8
Azote - -	40.7
Hydrogen -	24.5
	100.0

Having finished my analytical investigations, I pass on to the last divison of my subject which is the following comparative view of the composition of the prussic, ferruretted chyazic, and sulphuretted chyazic acids, and of their salts, as deduced from my analytical experiments, and as inferred from the atomic theory.

I was very well aware of the probability of my placing some of my analyses in a very unfavourable light, by contrasting the results obtained by the application of a theory, capable of giving the composition of bodies with absolute certainty, with those results which I have obtained by practical experiments, on a class of bodies hitherto little examined,

or understood, and the analyses of which were very difficult: but I would not allow this consideration to have any influence in deterring me from making such a contrast, for as I had not the vanity to give these analyses as perfect, so I feel no mortification in now proving, that they were not so; and being confident that I had not spared either time or trouble in making them, I expose their imperfections without hesitation, confiding in the candid judgment of those, who, having undertaken similar investigations, are aware of the numerous difficulties, and sources of error attendant upon them.

I have arranged and collected these comparisons into the form of a Table, which I beg leave now to introduce.

I infer from the Table, that the acids and salts included in it, are so composed as to harmonize perfectly with the doctrines of DALTON and BERZELIUS, and to be very compatible with the opinion respecting the compound nature of azote.

I shall be happy if this attempt to elucidate the nature and composition of these bodies, adds in any degree to the daily and rapid progress now making in chemical science.

ROBERT PORRETT, Jun.

Tower, Feb. 22, 1815.

TABLE showing in what degree the results of my analyses coincide with the atomic theory of DALTON, and with the law regulating the combinations of oxidized bodies discovered by BERZELIUS.

		Azote.	Carbon.	Hydrogen.	Prussic acid.	Red oxide of mercury.	Prussiate of mercury.	Sulphur.	Sulphuretted chyazic acid.	Pretoxide of copper.	Sulphuretted chyazate of protoxide of copper.	Peroxide of copper.	Sulphuretted chyazate of peroxide of copper.	Barytes.	Sulphuretted chyazate of barytes.	Black oxide of iron.	Ferruretted chyazic acid.	Potash.	Water.	Ferruretted chyazate of potash.	Peroxide of iron.	Ferruretted chyazate of black oxide of iron.	Ferruretted chyazate of peroxide of iron.	Ferruretted chyazate of barytes.	Atoms of oxygen. In the prussic acid: Upon the supposition that azote is a simple body.	Atoms of oxygen. In the prussic acid: Upon the supposition that azote is an oxide of an unknown radical	Atoms of oxygen. In the oxide which is an essential part of an acid.	Atoms of oxygen. In the oxides which act as bases in the salts.	Atoms of oxygen. In the water contained in the salts.
1 Prussic acid.	By analysis per cent.	40.7	34.8	24.5	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	By theory per cent.	40.7047	34.8169	24.4784	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	By theory per weight of atom.	1 atom	2 atoms	8 atoms	1 atom																								
		17.56	15.02	10.56	43.14	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	—	—
2 Prussiate of mercury.	Analysis per cent.	—	—	—	13.8	86.2	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	13.7766	86.2234	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
					1 atom	1 atom	1 atom																						
	Theory per atom	—	—	—	43.14	270.	313.14	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	2	—
3 Sulphuretted chyazic acid.	Analysis per cent.	—	—	—	34.8	—	—	65.2	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	35.0333	—	—	64.9667	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
					1 atom			4 atoms	1 atom																				
	Theory per atom.	—	—	—	43.14	—	—	80.	123.14	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	—	—
4 Sulphuretted chyazate of protoxide of copper.	Analysis per cent.	—	—	—	—	—	—	—	37.15	62.85	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	40.6215	59.3785	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
									1 atom	2 atoms	1 atom.																		
	Theory per atom	—	—	—	—	—	—	—	123.14	180.	303.14	—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	2	—
5 Sulphuretted chyazate of peroxide of copper.	Analysis per cent.	—	—	—	—	—	—	—	34.73	—	—	65.27	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	38.11	—	—	61.89	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
									1 atom			2 atoms	1 atom.																
	Theory per atom	—	—	—	—	—	—	—	123.14	—	—	200.	323.14	—	—	—	—	—	—	—	—	—	—	—	none	1	—	4	—
6 Sulphuretted chyazate of barytes.	Analysis per cent.	—	—	—	—	—	—	—	30.7	—	—	—	—	69.3	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	38.7525	—	—	—	—	61.2475	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—
									1 atom					2 atoms	1 atom														
	Theory per atom	—	—	—	—	—	—	—	123.14	—	—	—	—	194.62	317.76	—	—	—	—	—	—	—	—	—	none	1	—	2	—
7 Ferruretted chyazic acid.	Analysis per cent.	—	—	—	63.79	—	—	—	—	—	—	—	—	—	—	36.21	100.	—	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	66.569	—	—	—	—	—	—	—	—	—	—	33.431	100.	—	—	—	—	—	—	—	—	—	—	—	—
					4 atoms											1 atom.	1 atom												
	Theory per atom	—	—	—	172.56	—	—	—	—	—	—	—	—	—	—	86.66	259.22	—	—	—	—	—	—	—	none	4	2	—	—
8 Ferruretted chyazate of potash.	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	47.66	39.34	13.	100.	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	45.706	42.318	11.976	100.	—	—	—	—	—	—	—	—	—
																	1 atom	4 atoms	6 atoms	1 atom.									
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	259.22	240.	67.92	567.14	—	—	—	—	none	4	2	4	6
9 Ferruretted chyazate of black oxide of iron.	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	32.559	55.385	—	12.056	—	—	100.	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	34.632	51.796	—	13.572	—	—	100.	—	—	—	—	—	—	—
																2 atoms	1 atom		6 atoms										
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—	—	—	—	173.32	259.22	—	67.92	—	—	500.46	—	—	none	4	2	4	6
10 Ferruretted chyazate of peroxide of iron.	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	53.38	—	11.62	—	35.	—	100.	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	49.8059	—	13.05	—	37.1441	—	100.	—	—	—	—	—	—
																	1 atom		6 atoms		2 atoms		1 atom						
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	259.22	—	67.92	—	193.32	—	520.46	—	none	4	2	6	6
11 Ferruretted chyazate of barytes.	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—	—	49.1	—	—	34 31	—	16.59	—	—	—	—	100.	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—	—	49 629	—	—	33.051	—	17.320	—	—	—	—	100.	—	—	—	—	—
														4 atoms			1 atom		12 atoms										
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—	—	389.24	—	—	259.22	—	135.84	—	—	—	—	784.3	none	4	2	4	12

1 Page 229 of this Paper.
2 Page 223 of this Paper.
3 Phil. Trans. for 1814, page 549, C.
4 Ditto, page 555, B.
5 Inferred from the Analysis of the former salt.
6 Phil. Trans. 1814, page 555, B.
7 Phil. Trans. 1814, page 534, B and C.
8 Ditto, page 534.
9 Inferred from the analysis of the next salt.
10 Phil. Trans. 1814, page 537, A, B, and D.
11 Ditto, page 535.

XV. *On the nature and combinations of a newly discovered vegetable acid; with observations on the malic acid, and suggestions on the state in which acids may have previously existed in vegetables. By* M. Donovan, *Esq. Communicated by W. H. Wollaston, M. D. Sec. R. S.*

Read June 1, 1815.

Having often observed the sour disagreeable taste of the berries of the Sorbus (or Pyrus) Aucuparia, it occurred to me that the juice might contain an acid of a peculiar nature, and I resolved to submit it to a strict examination. I was not at that time acquainted with the fact, that these berries had already occupied the attention of Scheele, and that this philosopher had pronounced their acid to be the Malic.

Some preliminary experiments showed me that the juice occasioned a precipitation in acetate of lead and nitrate of silver; but it produced no change in lime water, barytes water, sulphate of copper, nor, although it might be expected from its very astringent taste, did it affect sulphate of iron.

A quantity of the juice was poured into a solution of sugar of lead; a curdy and somewhat heavy precipitate appeared, which was separated by filtration: this, from its solubility in acetic and dilute nitric acid, I judged to be malate of lead. The filtered liquor was red, and perfectly transparent, but after a few hours, I observed a powder deposited at the bottom, and as I saw no sufficient reason for its appearance, it

attracted my attention. To the naked eye it looked like a coarse powder; but, when examined by the microscope, proved to be composed of amorphous crystalline grains. With this small quantity of powder I made some trials, which ended in exciting farther curiosity. It was probable, that the precipitate which remained on the filter might be made to furnish more grains, and accordingly, after edulcorating the mass, I caused boiling water to pass through it. After two hours I examined the different washings; those made with cold water remained as at first, but those with boiling water had become white and turbid, from the suspension of a subtile powder. In several hours after, the cloud had disappeared, and displayed an abundant and beautiful formation of slender prismatic crystals, which glistened with silvery splendor at the bottom of the vessel. The mass which remained on the filter had become hard, was much diminished, and was very dense.

I accounted for the production of crystals by supposing that the precipitate, whatever might be its composition, was insoluble in cold water, but soluble in very large portions of boiling water, and hence the crystalline deposition on cooling. Experiment, however, showed, that the residual hard mass, even when levigated, boiled with water, and filtered, would no longer afford crystals on cooling: and this was a sufficient objection to my supposition; for, if one portion of the compound be soluble, why not the whole?

I observed, that even when the precipitate newly obtained, was washed with portions of cold water until it no longer altered vegetable colours; yet the first, second, and sometimes the third washing with boiling water would, after the deposition of the crystals, immediately redden litmus. An

opinion now suggested itself, which the following trials greatly strengthened.

In order to obtain the acid of the saturnine precipitate, the latter was boiled with dilute sulphuric acid: the liquor became red and transparent, while the sulphate of lead subsided to the bottom. Imagining that I had now obtained the acid in a state of sufficient purity, although coloured with adhering vegetable matter, I thought to separate the sediment by filtration, but was disappointed: for the liquor came through very turbid, notwithstanding that the supernatant liquor had been transparent. It was apparent, therefore, that the sulphuric acid being in too small a quantity, had displaced but a part of the vegetable acid, that the latter dissolved the remaining part of its own combination with lead, and deposited it when the solution touched any thing cold, thus producing the turbidness. After some hours, the turbidness in the filtered liquor subsided, forming a stratum on the bottom of the vessel, over which lay a number of crystals.

The theory of the crystalline formation in the washings of the precipitate now became obvious. It appeared that when the saturnine compound was washed with cold water, no other effect than edulcoration was produced: but that hot water partially decomposed the mass into a super and a sub-salt, the former of which being soluble in boiling water, filtered through, but on cooling, deposited crystals of the neutral salt, while free acid was left dissolved in the liquor. The first washing contained most free acid, and therefore suffered least of the neutral salt to crystallize.

The red acid liquor, as has been stated, contained much lead, and this it was necessary to separate. The most unex-

ceptionable method appeared to be the transmission of sulphuretted hydrogen through the clear liquor. This was accordingly done, after having heated the acid liquor so as to redissolve the crystals and sediment. The resulting sulphuret of lead was filtered off, and the clear fluid was boiled for a length of time, to expel the superfluous gas.

Supposing now that I had obtained the pure acid, I began to form various conjectures as to its nature; in the midst of which I discovered, that the berries of the Sorbus Aucuparia had already occupied the attention of Scheele, and that he had pronounced their acid to be the malic. There was indeed a great coincidence of properties between the two acids: malic acid is red, when evaporated to dryness it deliquesces, its combinations with potash, soda, and ammonia are deliquescent; such were also the properties of the acid under consideration. Yet I had never understood that malate of lead could be made to afford crystals: an experiment on this head, therefore, became necessary.

The juice of nearly ripe apples was saturated with potash, and the solution when filtered, was mixed with solution of acetate of lead: the precipitate was collected on a filter. This after being edulcorated was washed with boiling water, as before. In sixteen hours, crystals, precisely the same as the berries had afforded, were deposited, although less in quantity.

The production of crystals in both these cases, seemed to show that the acid of both fruits was the same: yet there was one difference. The precipitate remaining on the filter, after the action of boiling water, was, in that produced from apples, soft and pasty; but that obtained from the Sorbus berries was hard and dense. It became therefore necessary,

to discover what would be the habitudes of malate of lead obtained by other means.

SCHEELE showed that the primary action of nitrous acid on sugar is to form malic acid. I therefore heated together equal weights of these substances, until the effervescence ceased. The brown residue was diluted with lime water, and when the oxalic acid that might have been formed, was in this manner separated, the remaining acid substance was saturated with potash. Acetate of lead was added, and the malate of lead thus formed was collected and edulcorated.

It now remained to ascertain whether this malate possessed the property of crystallizing, like that prepared from apple juice. I accordingly poured on it different portions of boiling water which were received in different vessels: the washings were all of a brownish yellow colour, from a small quantity of malate of lead held dissolved. At the end of 48 hours this salt was all deposited in the state of a brown subtile powder, but there was no formation whatever of crystals. On the surface of each washing was an iridescent pellicle of some lustre, which I found to be characteristic of the malic acid. This experiment, as SCHEELE directs, was made with weak nitric acid; I repeated it with an acid of considerable strength, but after sixty hours there was not one crystal.

It deserves attention, that the matter which remained on the filter in these two experiments, after washing with boiling water, were as soft and pasty as when first collected; whereas the salt of lead obtained from the berries of the Sorbus, had grown dense, hard, and was much diminished.

The saturnine compound that had been formerly obtained

from the berries, when partially decomposed by sulphuric acid, afforded crystals on cooling: in order to ascertain if the malate of lead obtained from sugar would do the same, I repeated the process on it, but obtained no crystals. These experiments were made under almost every possible circumstance with the same results.

I next precipitated all the lead from the remaining malate; the malic acid thus obtained, was again mixed with acetate of lead, and the precipitate treated with boiling water as before: but the results were the same. From this malate of lead the acid was again separated, and the same process as before was four times repeated: but notwithstanding nearly all the foreign matter was thus separated, no crystals formed.

SCHEELE found that the gooseberry contains nearly equal parts of citric and malic acids. I thought it necessary to examine if this fruit would afford crystals. SCHEELE's process for separating the acids is as follows. The juice is to be evaporated to the consistence of honey, the pure acid to be separated by alcohol, and the alcohol to be finally evaporated. The residuum is to be dissolved in water, saturated with carbonate of lime; the solution is to be filtered, and boiled so as to separate the citrate of lime. The remaining brown solution is malate of lime: the salt is to be precipitated by alcohol, redissolved after edulcoration, and the solution is to be precipitated with acetate of lead.* All this I followed; but after treating the malate of lead with boiling water, I obtained no crystals.

The raspberry was found by SCHEELE to contain the same

* CRELL's Chemische Annal. 1785, vol. ii. 292.

acids. I examined this fruit in a similar manner, but no crystals were produced.

Vauquelin detected malic acid in a variety of plants, but in none so abundantly as the Sempervivum Tectorum. In the juice of this plant, it is united to lime in the state of a supersalt.*

Malate of lead was formed by pouring solution of acetate of lead into the filtered juice of this plant. The precipitate, when washed with boiling water, deposited on cooling, the the same subtile powder as usual, but no crystals. Nor were any obtained when the malate was partially decomposed by sulphuric acid, in the manner already described.

Scheele found that the berries of the Sambucus Nigra, contain the malic acid unmixed with any other. I therefore examined their very mucilaginous juice, but could not produce crystals.

As in my experiments on apples I had obtained crystals, I wished to try what effect the vinous fermentation might have on their juice. The process was repeated with cyder, but I was surprised to find, that although a malate of lead was obtained, the property of forming crystals had been destroyed.

From various conjectures it appeared possible, that the berries of the Sorbus while very young, might perhaps contain a malic acid that would furnish few or no crystals. In the beginning of August (1812) the berries being still green, I made an infusion of them, which after filtration afforded a precipitate with acetate of lead. Boiling water produced no crystals, nor did the mass become hard as on former occasions.

Seven varieties of malic acid had now been examined,

* Annales de Chimie, Tom. xxxiv. 127.

which when united to lead would not afford crystals. In the two latter cases it was surprising that those acids which in other trials afforded abundance, would now afford none. That the juice of the green berries was real malic acid, was proved by the following properties.

1. The acid, when separated from the juice, was of a crimson colour.

2. When evaporated to dryness, it soon deliquesced.

3. When rendered solid, and acted on by nitrous acid, some crystals of oxalic acid appeared.

4. The acid formed deliquescent salts with potash, soda, and ammonia.

5. Its combinations with lime and lead possessed their proper characteristics.

The acid of very young sour apples was combined with lead, and the compound treated with boiling water: the washings deposited much more crystals than mature apples would have done.

The truth was now manifest. Beside the malic, there is another acid formation, which in different fruits is formed at different periods, and which has hitherto escaped observation: and I consider the preceding details by no means redundant, inasmuch as they establish one decided difference between the two acids. Many others will shortly be stated.

The first step towards confirming the difference, was to obtain the new acid in a state of purity: and after numerous attempts, I found that the only way to obtain it pure, was to separate it from the crystals. The process is indeed complex and difficult.

The berries of the Sorbus Aucuparia are to be collected

when first they have arrived at maturity. After sufficient bruising in a Wedgewood mortar, they are to be subjected to strong pressure in a linen bag. If collected at the most favourable time, they will afford nearly one half their weight of juice, S. G. 1077. This after due subsidence is to be strained, and mixed with filtered solution of acetate of lead. The precipitate is to be collected on a filter, and in order to separate any uncombined colouring matter, it should be washed with cold water. A very large quantity of boiling water is to be poured on the filter, and allowed to pass through the precipitate into different glass jars. After some hours, the washings become opaque, and at length deposit crystals of singular lustre and beauty. Those which have been formed in the colourless washings are to be alone retained; they are to be separated by the filter, dried in the air, and preserved for a farther process.

The original mass remaining on the filter, from which the crystals have been obtained, being now hard and brittle, is incapable of affording any more, without undergoing a new operation. It is to be boiled for half an hour with a little more dilute sulphuric acid than is sufficient to decompose the salt; when cold it is to be filtered. The filtered liquor is to be mixed a second time with acetate of lead; the precipitate washed, as before, with boiling water, and the crystals selected from the colourless washings only. The remaining mass again grown hard, is to undergo the process of decomposition with sulphuric acid, combination with lead, and the formation of crystals: and after all, it will be found that the crystals of all the processes will be inconsiderable when collected.

The whole of the crystalline product being dried, is to be

boiled for half an hour with 2.3 times its weight of sulphuric acid, s. g. 1090, supplying water as fast as it evaporates, and taking care to keep the mixture constantly stirred with a glass rod. The clear liquor is to be filtered off, aud poured into a tall glass jar of small diameter. While still hot, a stream of sulphuretted hydrogen is to be transmitted through it, and when all the lead has been precipitated, the fluid is to be filtered off, and boiled in an open basin, until the discharged vapour no longer blacken paper impregnated with acetate of lead.

The theory of the process is obvious. When acetate of lead is added to the juice, malate of lead and the combination of the new acid with lead precipitate ; the latter is decomposed by boiling water into a super and a sub-salt; the supersalt is held in solution, but as the liquor cools, the neutral compound deposits itself in crystals, and the first washings contain most free acid. When boiling water is no longer able to overcome the attraction of the latter portions of acid to oxide of lead, no more crystals can be formed. We then apply the stronger power of sulphuric acid, we obtain the free vegetable acid, and proceed as at first. When all the crystals are collected, such a quantity of sulphuric acid is added as will be *nearly* sufficient to decompose them: this is so done in order completely to exclude the sulphuric acid, which without this precaution would be exceedingly difficult to effect. The undecomposed portion of the crystals dissolves in the vegetable acid newly extricated: but, if in the boiling, the fluid were not continually stirred, a mass would be formed in the bottom so hard as to resist decomposition. If the liquor after filtering were allowed to cool, the neutral salt

would crystallize; it must therefore be used hot. The stream of sulphuretted hydrogen passing through so high a column of fluid soon separates the lead, while the pure vegetable acid is liberated, contaminated indeed with a little sulphuretted hydrogen. This gas does not disappear completely by boiling, for the acid retains the odour, be it ever so long boiled; exposure for a few days in an open vessel dissipates it completely.

In preparing this acid, it is not necessary, as it is in the process for malic acid, to saturate the juice of the berries with potash, at the commencement of the process: for of the two compounds formed after the mixture with acetate of lead, the malate dissolves in the evolved acetic acid in preference to the other. The colouring matter, which adheres obstinately to the malate of lead, is very apt, when extricated during the washing with boiling water, to tinge the otherwise perfectly colourless crystals, which form as the liquor cools. This is a great inconvenience, for the colouring matter cannot be washed away, even by cold water, without decomposing a quantity of the salt: hence the only remedy is, to reject all the crystals formed in the coloured washings, and to reserve those only that are of a pure whiteness. The crystals are of so delicate a fabric that they must be separated by the filter. When dried on paper, by exposure to air, they form a white brilliant flake of a silver lustre, resembling well-prepared acetate of mercury, but still more beautiful.

As to the amorphous crystalline grains which first attracted my attention, the following experiment elucidates the theory of their formation. A quantity of the pure acid obtained by the above process, was boiled for some time on an excess of

carbonate of magnesia. The liquor after filtration was found to restore the original colour of reddened litmus, and to render green the tincture of cabbage. An acetate of lead was formed by boiling solution of super-acetate on carbonate of lead. This solution was mixed with the former, and the precipitate was collected by the filter.

Notwithstanding the evident excess of magnesia existing in one solution, and the necessary neutrality of the other, yet the filtered liquor was found strongly to redden litmus paper. We are not to suppose that the two salts evolved a free acid during their mutual decomposition. As much oxide of lead was liberated from the acetate, as was necessary to the neutrality of the acid eliminated from the compound with magnesia; the solution would therefore have retained all its ingredients in a state of neutrality, except that which originally contained an excess of magnesia. But the new salt of lead at the moment of its formation, was decomposed by the water present into a super and a sub-salt: the excess of acid being not only sufficient to saturate the redundant magnesia, but also to leave a portion free in the solution. This liquor, after an hour, deposited a quantity of crystalline grains, and after that, the acidity increased. Hence appears the reason of a crystallization in the original liquor: a super-salt is formed, which after a while deposits the neutral salt in a crystalline form.

This acid appearing from what has been already stated, as well as from what will be hereafter detailed, to be of a peculiar nature, it became necessary to give it a name. After some consideration I bestowed on it one, which, although not unexceptionable, is sufficiently accordant with the general

analogy of chemical nomenclature, and which has received the approbation of some competent judges. Until a better name be devised, I have called it the Sorbic Acid.

To establish its peculiar nature, I have examined its combinations with certain bases, but have confined myself to those of which the analogous combinations amongst the malates had been already examined by SCHEELE. The sorbic and malic acids not having been distinguished by that philosopher, it seemed that here the distinction ought particularly to be established: and the standard of comparison must necessarily be whatever had been ascertained of the malates by their discoverer.

Sorbic acid, when perfectly pure, is a transparent, colourless, and inodorous fluid, soluble in alcohol, and in any proportion of water. When evaporated, it forms an uncrystallizable solid mass which deliquesces: when subjected to distillation, the liquor which passes over, shows no traces of acidity. Its acidity is such that it causes even a painful sensation on the organs of taste. It is not much altered by being kept in an uncombined state. I have had it for more than a year in a corked phial, and at the end of that time, no other change was produced than the separation of a tenuous coagulum, small in quantity, as the acid was very pure, but it is more abundant when the acid is impure. When mixed with malic acid, as in fruits, this acid is the first to disappear, while the other retains its properties long after the commencement of decay in the plant.

A quantity of malate of lead obtained from Sempervivum Tectorum was boiled with sorbic acid and a little water; the whole, from being colourless, became somewhat brown. The

liquor was then filtered, and the turbid liquor which came through, was heated until it became clear; it was then suffered to rest. As it cooled, it let fall a powder, but when this was filtered off, the liquor remained clear, and in an hour a great profusion of crystals was let fall. The mass which remained on the filter contained some gritty particles.

Thus it is evident, that malate of lead was decomposed by sorbic acid, which could not happen unless the latter were a distinct substance. The malate was only partly decomposed, the oxide of lead united to the excess of sorbic acid forming super-sorbate of lead, while the disengaged malic acid dissolved as much as it could of the remaining undecomposed malate, forming super-malate of lead. The brown colour was produced by disengaged malic acid. The super-malate, as it cooled, deposited its malate in the state of powder, and the super-sorbate soon after deposited sorbate of lead in the state of crystals; and the original mass was found to contain gritty particles of sub-sorbate.

In the same manner, when a precipitate, obtained by acetate of lead from the juice of the Sorbus berries, is washed with boiling water, scarcely any malate of lead is deposited; and if the fluid contain much free sorbic acid, the iridescent pellicle, which is a characteristic of malate of lead, does not appear.

I shall now proceed to the combinations of this acid, so as to distinguish it from malic acid: and first the salts which it forms with lead should be briefly recapitulated.

The sub-sorbate is insoluble in water; if in a mass, it is dense and hard; if in powder, it is gritty.

The neutral sorbate, if obtained by precipitation, is a white powder, but if obtained from solution in its own acid, it is in

beautiful silvery crystals. Neither of these salts is soluble in 5000 times its weight of water. The sorbate when heated to redness, undergoes a somewhat brilliant combustion.

The super-sorbate never assumes the solid form; its taste is sweet. Thus the sorbic acid forms three combinations with lead: malic forms but two, the neutral malate, which is an uncrystallizable soft powder, and the super-malate. Not less distinguishable are the two acids by their combinations with the alkalies.

Sorbate of potash, when there is an excess of acid, forms permanent crystals soluble in water, but insoluble in alcohol.

Sorbate of soda, when there is an excess of acid, forms permanent crystals, which agree in characters with the former.

Sorbate of ammonia, when there is an excess of acid, also forms permanent crystals of similar characters with the preceding.

These three salts will not crystallize unless there be a tolerable excess of acid; they are to be considered as super-salts. That of soda even requires the aid of cold to render it solid. The malates of potash, soda, and ammonia are known to be uncrystallizable and deliquescent.

In the combinations of these acids with earths, there are also striking differences. SCHEELE found, that when he added carbonate of lime to malic acid, a great quantity was dissolved, but the solution gave with litmus, indications of an abundant acid which it was impossible to neutralise with farther additions of chalk.* I obtained the same result with malic acid from the Sempervivum; I even found that the solution might be boiled to dryness, on a fresh portion of carbonate of lime,

* SCHEELE, Chem. Annal. 1785. 2. 292.

yet when lixiviated, the filtered solution would still redden litmus, and the salt finally afforded was readily soluble. These results often obtained, prove that it is not possible to form neutral malate from carbonate of lime.

But with sorbic acid the case was quite different. When it was diluted, and agitated for a little while with carbonate of lime, the solution, before it could be filtered, deposited the principal part of the sorbate in the form of a discrete, gritty powder. The liquor when filtered produced no redness in tincture of litmus, and every thing proved that the fluid by mere agitation over the carbonate, had been completely neutralised.

The same results which SCHEELE obtained from lime were afforded by carbonate of barytes: but with sorbic acid I produced a liquor which showed no signs of acidity. The best test for ascertaining this fact, seemed to be infusion of brazil wood altered by distilled vinegar; and with this it even appeared, that the solution contained an excess of base.

Thus it appears, that malic acid never forms with carbonate of lime, any other than *acidulous salts;* and, as SCHEELE observes, these solutions in *some days* deposit the neutral salt in *crystals.* But with these carbonates, the sorbic acid forms *neutral salts,* which, *as soon as formed,* precipitate.

SCHEELE ascertained, that malate of magnesia is a deliquescent salt,* and in my trials I could not obtain it in a crystalline form. When evaporated, it became thick, and dried into a semitransparent substance, which softened with the smallest quantity of water, and formed matter of a syrupy consistence. The same earth, heated in sorbic acid, afforded a

* CRELL's Chem. Annal. 1785. 2. 297.

liquor which, after filtration, deposited permanent crystals in abundance: they required for solution no less than 28 parts of water at 60.*

The malate of alumina was found by SCHEELE to be a salt very difficult of solution. I wished to discover the properties of the sorbate. I therefore boiled some very pure alumina that had been just prepared, and was therefore still soft, with sorbic acid: the boiling was continued for almost an hour, and after filtration, I discovered with no small surprise, that the alumina had not been acted upon. The acid was tried by every means, and nothing but the most minute vestiges of the earth could be obtained. Thus there is no sorbate of alumina.

I consider that from this property the sorbic acid may become a valuable instrument of analysis. The process for separating alumina from other earths, has been complicated and uncertain: may it not be rendered simple by the use of this acid, employed in excess?

Thus, I think there can be no doubt, that the sorbic acid is an acid *sui generis*, and probably intermediate between malic and oxalic. With regard to the other acids, with which the sorbic coexists in fruits, it is to be observed, that it is never found in mature fruits that contain any other than the malic; that the latter is never found alone in any mature fruit, but always accompanied by the sorbic, and that these two acids, when together, exclude all others. To this, however, there is an apparent exception, namely, the berry of the Sambucus Nigra, which (probably from the immense quantity of mucilage and

* It deserves remark, that in SCHEELE's experiments, there could have been no sorbic acid present, as might have been expected, had he prepared his acid from apples: he obtained it from gooseberries, and thereby avoided this source of fallacy.

colouring matter present) afforded me no sorbic acid. The fruits that contain the sorbic and malic acids together are apples, plums, berries of the sorbus, barberries, and sloes. Of these, the berries of the sorbus contain the greatest quantity of sorbic acid, unripe apples less, ripe apples and sloes still less, barberries very little, and plums least of all. The green berries of the sorbus, (perhaps,) those of the sambucus, and the plant sempervivum tectorum, contain no other than the malic; and agreeing with the foregoing statements, raspberries and gooseberries, as they contain citric and malic acids, contain no sorbic whatever.

Observations on the malic acid.

In 1785, during an examination of different fruits and berries, Scheele discovered that gooseberries, beside lemon acid, contained one of a peculiar nature: this he afterwards found to exist in apples, without, as he thought, a sensible admixture of any other. On this account he gave it the name of apple acid, or malic acid.

He also ascertained, that by the action of nitrous acid on sugar, a substance is produced, which shows no traces of nitric acid, yet unites and forms a soluble salt with lime, "it therefore is not the oxalic acid." By some other experiments he found that an acid is produced "which does not differ in the least from the properties of the apple acid, and is accordingly the same."

This acid he detected in a great variety of vegetable juices. Since that period, Vauquelin has extended the catalogue, but of all other plants, it is most abundantly contained in the Sempervivum Tectorum.

Scheele's process for obtaining malic acid is as follows.

" Saturate the juice of apples, whether ripe or unripe, with carbonate of potash; add solution of acetate of lead until it cease to produce a precipitation. To the edulcorated precipitate, add as much dilute vitriolic acid, as is necessary to give the mixture a perfectly acid taste, without any sweetness."*

There are several objections to this process, all of which seem to have considerable weight. In the preceding pages I have shown, that the juice of apples, whether ripe or unripe, always contains two acids of very different properties. By the above process these acids are not separated; they are in fact found in what is supposed to be the resulting pure malic acid, and it is impossible, without the most complicated processes, to obtain this substance in the insulated form.

The precipitation of the lead by means of sulphuric acid, appears to be objectionable. I have often attempted to adjust the proportion of the latter substance, so as to throw down all the lead, without leaving any free sulphuric acid, but I uniformly failed: and it is evident, that, if not impossible, it is exceedingly difficult and troublesome.

Scheele also attempted to obtain malic acid from malate of lime, by means of sulphuric acid, but found " the mode rather difficult, as the acid would not let the calx fall completely." Vauquelin observed the same thing.

The last process proposed by Scheele, is to distil equal parts of weak nitric acid and sugar, until the mixture become brown, which is a sign that all the nitrous acid has been abstracted: the oxalic acid formed, is to be separated by lime water, and the remaining acid to be saturated with carbonate of lime.

* Crell's Chemische Annal. 1785. Vol. II. 295.

The solution is to be filtered, the filtered liquor to be mixed with alcohol, and the coagulum thus obtained, is to be edulcorated with new portions of alcohol. The coagulum is then to be dissolved in water, and mixed with a solution of acetate of lead: a precipitate falls, which is to be treated with sulphuric acid, in the manner already directed.

No one who has not gone through this process, can fully conceive the difficulty and expense of it: and I have found that the acid obtained is variable in its nature. In one case I obtained an acid, which, when mixed with solution of acetate of lead, did not at first produce any effect, but at length slowly deposited a precipitate. The heating of another portion of the acid with carbonate of lime, produced a separation of a black powder, which possessed the properties of charcoal. There were also many other peculiarities; and the combined effect of all was to convince me, that great differences exist between the acid obtained in this manner, and that obtained by other processes.

The experiments of VAUQUELIN satisfied him that the acid which is combined with lime in the Sempervivum Tectorum, is the true malic: and all my trials convince me, that it does not contain even the least quantity of the sorbic. Since then, by the means generally employed, we do not obtain malic acid, the only alternative is to adopt the hitherto neglected process of VAUQUELIN; and it will be found that his process affords the acid with greater ease, and in much greater purity, than any other. The method of detaching the acid from the malate of lead by sulphuric acid is, as we have seen, difficult; and the criterion of the taste is liable to this fallacy, that as the sourness increases, the sweetness decreases. There will

on this account, be a period when the latter will disguise the former, and yet the lead will be still present. I would therefore suggest the substitution of sulphuretted hydrogen in place of sulphuric acid.

If it were required to obtain malic acid exceedingly pure, and still more divested of vegetable matter, the following process may be adopted.

The juice of Sempervivum Tectorum is to be evaporated to two-thirds, and, after standing some hours, it is to be filtered, and mixed with an equal quantity of alcohol. The coagulum is to be separated by the filter, edulcorated with fresh portions of alcohol, and dried in the air, lest any adhering alcohol should impede its subsequent solution. The mass is then to be dissolved in water, mixed with solution of acetate of lead, and the precipitate collected on a filter. After being well edulcorated from any superfluous acetate of lead, the precipitate is to be boiled for 15 minutes with a little less of dilute sulphuric acid than is sufficient to saturate the oxide of lead: and for this part of the process, the criterion of sweetness will very well answer the purpose. The whole is to be set aside for some days, and, during this period, a small quantity of sulphate of lead which the malic acid held dissolved, will be deposited. The liquor is now to be filtered, and in order to separate the last portions of lead, a stream of sulphuretted hydrogen is to be transmitted through it: the black precipitate is to be filtered off, and the liquor should be boiled in a vessel freely exposed, until paper moistened with acetate of lead is no longer blackened by the discharged vapour. This acid is the purest that can be obtained; it retains

a slight odour of the gas, but even this is destroyed by exposure to the air for a few days.

VAUQUELIN observes, that malic acid thus obtained is nearly colourless: his was therefore diluted. I have found that it becomes perfectly brown by concentration: and I have decomposed and recomposed malate of lead several times, using each time the same specimen of malic acid, yet so obstinately did the colouring matter adhere, that it was always found in the resulting acid. Thus, as far as we know, this acid cannot be procured free from colour; and the nearest approximation is that obtained by VAUQUELIN's process.

Suggestions concerning the state in which acids may previously have existed in vegetables.

I have sometimes indulged in the supposition, that the vegetable acids are not primarily formed by the immediate union of their elements, but that they may have previously existed in a definite combination, called the bitter principle. It is possible that this principle may be a compound basis, which by uniting to oxygen, or by undergoing more complicated processes, might change its nature so far as to become an acid. The whole is a mere conjecture, and perhaps deserving of little consideration; the facts, however, which suggested it may be noticed.

The sweetness of any vegetable juice, has been generally attributed to a sweet principle called sugar. In the same manner it has been lately supposed, that bitterness depends on a bitter principle, which, although variously disguised, is always identical. Dr. THOMSON has shown, that when

water is digested over Quassia, and afterwards evaporated to dryness, a transparent substance is obtained, which differs in its properties from all other vegetable principles: this he considers as the bitter principle, and, I believe, with very great justice. I found that the liquor obtained by digestion, although slightly coloured, was transparent even to the end of the evaporation. The resulting mass was nearly transparent, and minute in quantity, considering the proportion of Quassia employed; and such was its bitterness, that a particle placed on the tongue, which could not have exceeded $\frac{1}{50}$th of a grain, diffused an intense bitterness over the whole mouth and fauces.

This matter was heated with nitric acid; it dissolved with effervescence, and the bitterness was no longer sensible. The remaining substance formed a precipitate in acetate of lead, which possessed all the properties of malate of lead: and it appeared that no other than the malic acid was produced. With this experiment the following agrees in a remarkable manner.

Five ounces of white sugar, and an equal weight of very strong nitric acid were mixed in a retort. Without the application of external heat the action commenced, and soon became violent. When cold, the residual matter was found to be thick and tenacious; its taste was sour, and extremely bitter. The malic acid being abstracted from a portion of this by means of lime, it was found that the bitterness, now no longer disguised by acidity, had become intense. The other portion, which had not been saturated with lime, by being treated with more nitric acid, lost all its bitterness, and oxalic acid was formed. In this experiment it appears, that by some

action of nitrous acid on sugar, a bitter substance and malic acid were produced together; that by the farther action of nitrous acid, the bitter substance disappeared, and acid appeared in its stead.

The foregoing conjectures correspond also with the fact, that by the action of certain substances on each other, the bitter principle is evolved at the same time with those acids which I suppose to have been produced from that compound basis: and the appearance of both at the same time may be accounted for by admitting that the conversion was not complete. Thus, if alcohol be distilled with nitrous acid, a liquor is produced which has a sweet taste. If this liquor be re-distilled with another portion of acid, a bitter liquor comes over. And if this bitter liquor be distilled a third time with a fresh portion of nitrous acid, crystals of oxalic acid make their appearance in the residuum. This series of changes bears a striking resemblance to that produced by the action of nitrous acid on sugar.

HAUSSMAN observed, that when nitric acid is digested with indigo, a very bitter substance results, to which WELTHER gave the name of Amer: in this process, oxalic acid is also formed.

The vegetable acids are even formed by the action of nitrous acid on animal substances; in the instance of muscle we obtain the abovementioned Amer with oxalic acid. In bile the bitter principle is already formed; when acted on by nitrous acid, oxalic acid is produced.

On examination we shall not be at a loss to find operations analogous to some of the preceding, taking place naturally in the vegetable kingdom. The Pyrus Malus or common

crab apple, while young, is very bitter, and has little sourness: as the fruit advances towards maturity, the taste becomes proportionately sour, and the bitterness diminishes. The young berries of the Sorbus Aucuparia also are bitter, contain but one acid, and even that in small quantity: when the berry is ripe, it contains two acids, the combined quantity of both is considerable, but the bitterness has in a great degree given place to a coarse astringency.

It is not improbable, that the bitterness produced in all the foregoing cases, should be owing to the formation of the same bitter principle: and its constant conjunction with a vegetable acid, seems to show that there is some very intimate connexion between them, at present unknown.

The preceding observations are offered as mere conjecture; and I am fully sensible of what little consideration should be attached to them: they are not however entirely devoid of probability. An hypothesis is below the dignity of a system which is founded on the indestructible basis of experiment: and even though it be supported by the coincidence of admitted facts, by direct analogies, and by the correspondence of received opinions, it should nevertheless be the beginning and not the end of knowledge.

XVI. *On the structure of the organs of respiration in animals which appear to hold an intermediate place between those of the class pisces and the class vermes, and in two genera of the last mentioned class.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read June 1, 1815.

From all the facts in comparative anatomy with which we are acquainted, there is reason to believe, that the great scheme of the animal creation is composed of one uniform gradation of structures, and it is only by collecting the different appearances met with in the same organs of different animals, into regular series, that a solid basis can be laid on which a general system may be constructed.

In this view of the subject, every link that is added to any one series, acquires a value; as it increases, in however small a degree, the foundation upon which an edifice of such importance is to be raised, and therefore may not be undeserving of the attention of the Society.

In fishes, the mode of respiration by means of gills is well understood, and there is probably no better criterion by which an animal may be allowed to belong to that class, than its having gills. In the class vermes, confining our observations to those genera that live under water, the respiratory organs commonly met with, are of two kinds, one internal as in the genus Teredo, the other external as in the genus Amphitrite, both of which I have described to the Society upon a former occasion.*

* See Phil. Trans. Vol. lxxv. p. 333.

The materials of the present Paper include five several links in the chain distinct from the gills of fishes, and different from the organs common to vermes; these are met with in the lamprey and lampern, in a new genus intermediate between the lamprey and myxine, in the myxine, in the aphrodita aculeata, and in the leech.

I shall first give a short description of each of these organs, and afterwards explain the modes of respiration.

In the lamprey the organs of respiration have seven external openings on each side of the animal; these lead into the same number of separate oval bags placed horizontally, the inner membrane of which is constructed like that of the gills in fishes. There is an equal number of internal openings leading into a tube, the lower end of which is closed, and the upper terminates by a fringed edge in the œsophagus. These bags are contained in separate cavities, and inclosed in a thorax resembling that of land animals, only composed of cartilages instead of ribs, and the pericardium, which is also cartilaginous, is fitted to its lower extremity like a diaphragm.

In the middle line of the anterior part of the thorax are situated the muscles of the tongue, forming one solid mass, from which a distinct muscle is continued down to the pericardium, sending off fasciculi to the cartilages in the lower part of the thorax.

There is but one nostril, which opens into a cavity of considerable size, having no posterior opening. Where the œsophagus terminates in the stomach, it adheres to the pericardium, and forms an oblique valvular slit, which is closed by the dilatation of the stomach. There is no gall bladder.

In the lampern, the structure of these organs is the same

as in the lamprey, only the cartilages of the thorax are so weak as to appear like ligaments, and the pericardium is membranous.

In an animal brought from the South Seas by Sir JOSEPH BANKS, intermediate between the lamprey and myxine, but differing so much from both as to form a distinct genus, the respiratory organs resemble those of the lamprey in the number of the external openings, and the number of bags, but these organs and many other parts differ in the following particulars, in which they agree with those of the myxine. There is no appearance whatever of thorax, nor is the pericardium cartilaginous; the bags are flattened spheres placed perpendicularly, their cavities are small, their coats elastic, and the internal orifices communicate directly with the œsophagus, which is small. The œsophagus does not terminate in a valvular slit, but in a loose transverse membranous fold; there are two rows of teeth on each side of the tongue, bent downwards, long, and pointed. There is a posterior nostril, and an appearance resembling an uvula. There is a gall bladder, a row of large mucous glands on each side of the belly, and there is a mesentery to the intestine.

In the myxine, the respiratory organs differ from those last described, in there being only two external openings, and six lateral bags on each side, to which there are six tubes from each of the openings, and, close to the left external opening, there is one which passes directly into the œsophagus, the gall duct projects into the intestine.

In the aphrodita aculeata of LINNÆUS, the respiratory organs as well as the other viscera, differ in many respects from those of all the other animals of that tribe. There are thirty-two open-

ings on each side in the intermediate spaces between the tufts of bristles; these all open into a large cavity immediately under the skin and muscles of the back, which is only separated from the cavity of the abdomen by a strong cartilaginous membrane, but there are two rows of spherical cells, fifteen in each, projecting into the cavity with very thin membranous coats. There is no external opening into them, but a slit on the under surface, by means of which, one of the cæca belonging to each of the tubes passing off from the intestine is lodged in each of the cells, which leads me to consider such cæca to be the respiratory organs.

In the common leech, there are sixteen orifices on each side of the belly, which lead to an equal number of spherical cells placed between the abdominal muscles and the stomach, which perform the office of respiratory organs. The particular structures which have been described, are represented in the annexed drawings, (Plates XI. XII. XIII.) which makes a more detailed verbal description unnecessary.

Having described the structure of the organs in these five different genera of animals, I shall endeavour to explain the manner in which respiration is carried on in each.

In the lamprey and lampern, the water is received by the seven lateral openings on each side of the animal into the bags which perform the office of gills, and passes out by the same orifices. The form of the cavities being fitted to allow the water to go in at one side, pass round the projecting parts, and out at the other. A part of the water escapes into the middle tube, and from thence, either passes into the other bags, or out at the upper end into the œsophagus. There is a common opinion that the water is thrown out of the nostril;

this, however, is unfounded, as the nostril has no communication with the mouth. The elasticity of the cartilages of the thorax, admits the water being received, and it is expelled by the action of the muscles drawing up the cartilages and the pericardium. The animal from the South Seas having no cartilaginous thorax, the bags themselves have an elastic covering which keeps them open to receive the water, and it is expelled by the action of the external muscles into the œsophagus.

In the myxine, the elasticity of the two tubes and the bags into which they open, admits of the water being received, and the pressure produced by the action of the external muscles forces it into the œsophagus, from whence it is thrown out by the opening at the lower end of that tube.

Bloch has given a correct account of many parts of the myxine, illustrated by engravings, but there are several errors respecting the mode in which the water passes out. He supposes it to be thrown out at the nostril. He was probably led into this mistake from finding a posterior nostril communicating with the mouth.

In the aphrodita aculeata, the water passes through the lateral openings between the feet into the cavity under the muscles of the back; it is there applied to the surfaces of the projecting cells, through which the air in the water is communicated to the cæca contained in them: these cæca I consider to be the respiratory organs.

In the leech, the water is received through the openings on the belly of the animal into the cells, or respiratory organs, and passes out by the same openings.

A knowledge of the mechanism employed for the pur-

poses of respiration in the sturgeon, and in the three first genera of animals mentioned in the present Paper, enables us to carry on a regular series of links, in gradation, from fishes in general to the myxine; every change in structure arising out of some peculiar habit of life, belonging to the animal in which it is met with.

In fishes, the gills are so formed, that the water forced from the mouth out at the gills, is applied to them in the most complete manner.

In the sturgeon, while swimming, respiration is carried on in the same manner, but when the sturgeon adheres to any substance by the mouth, which it has a power of doing by extending its lips, some other mode of respiration is required, and it is found that in the act of pushing out the mouth, apparently by the same means, the gill covers are drawn up so as to leave a large channel between them and the gills, through which the water is brought into the mouth and returned through the gills; there is also on the inside of the gill cover the same structure as on the side of the opposite gill, only to a smaller extent.

In the lamprey, the mouth is more constantly employed in laying hold of its prey and other substances, and therefore the respiratory organs are not connected with it, but situated near it.

In the myxine, which feeds upon the internal parts of its prey, and buries the head and part of its body in the flesh, the openings of the respiratory organs are removed sufficiently far from the head to admit of respiration going on, while the animal is so employed.

The respiratory organs in the two last genera mentioned

in this Paper, belong to a series of less complex structures, and perhaps, few animals can have a more simple mechanism than the leech.

EXPLANATION OF THE PLATES.

Plate XI.

A view of a portion of the lamprey of the natural size, the organs of respiration exposed.

The mouth is laid open, exposing the teeth.

a. The tongue, on which there are teeth turned on one side.

b. The cavity of the mouth.

c. The fauces.

d. The tube between the bags containing gills.

e. Its termination in a loose edge at the orifice of the œsophagus.

f. A firm cartilage in the centre of the retractor muscles of the tongue.

gg. Two large salivary glands.

hh. The cavities containing a structure like gills laid open through their whole extent.

ii. The external orifices of these cavities.

kk. The internal orifices.

ll. The cartilages of the thorax.

m. The cartilaginous pericardium.

n. The termination of the œsophagus in the stomach.

Plate XII.

The respiratory organs in an animal from the South Seas, and in the myxine.

Fig. 1. These organs exposed in the animal from the South Seas.

a. The external nostril.

b. Internal nostril.

c. A tooth on the roof of the mouth.

dd. The tongue split, showing two rows of teeth on each side.

ee. The muscles of the tongue divided and turned aside.

ff. The œsophagus.

gg. The external openings into the respiratory organs.

hh. The internal ones,

ii. The organs themselves.

Fig. 2. One of the bags or organs laid open.

Fig. 3. The same parts in the myxine.

a. The external nostril.

b. The internal nostril.

c. A tooth in the roof of the mouth.

dd. The tongue split, showing two rows of teeth on each side.

ee. The muscles of the tongue turned aside.

ff. The œsophagus.

g. The stomach.

hh. The two external openings leading to the organs.

i. The opening leading to the œsophagus.

kk. The tubes leading to the organs.

ll. The internal openings.

mm. The organs themselves.

nn. The mucous glands.

Plate XIII.

The respiratory organs in the aphrodita aculeata and leech.

Fig. 1. Back view of the aphrodita aculeata.

Fig. 2. The respiratory organs laid bare by removing the skin and muscles of the back.

aa. The cells projecting into the cavity under the muscles of the back.

bb. The external openings leading into the cavity.

c. The gizzard exposed, the thin cartilaginous covering seen on the opposite side having been removed.

d. The intestine.

ee. The lateral tubes going off on each side.

ff. The cæca, which project into the cells.

Fig. 3. The hirudo medicinalis laid open from behind, the stomach removed, exposing the respiratory organs, consisting of thirty-two transparent cells, in each of which the external orifice is seen through the coats.

The spinal marrow with its ganglia and nerves distinctly seen.

aa. The respiratory cells.

bb. A large blood vessel on each side.

cc. Mucous glands.

dd. Glandular structures communicating with the testicles.

ee. The testicles.

f. The penis.

g. The uterus.

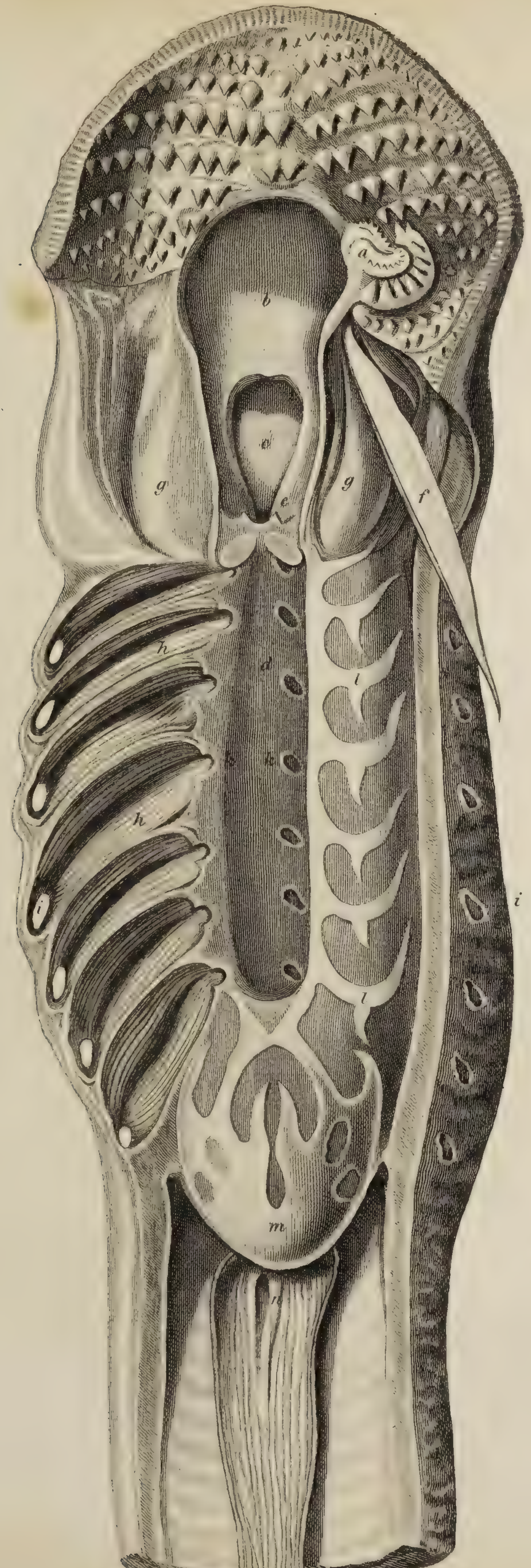

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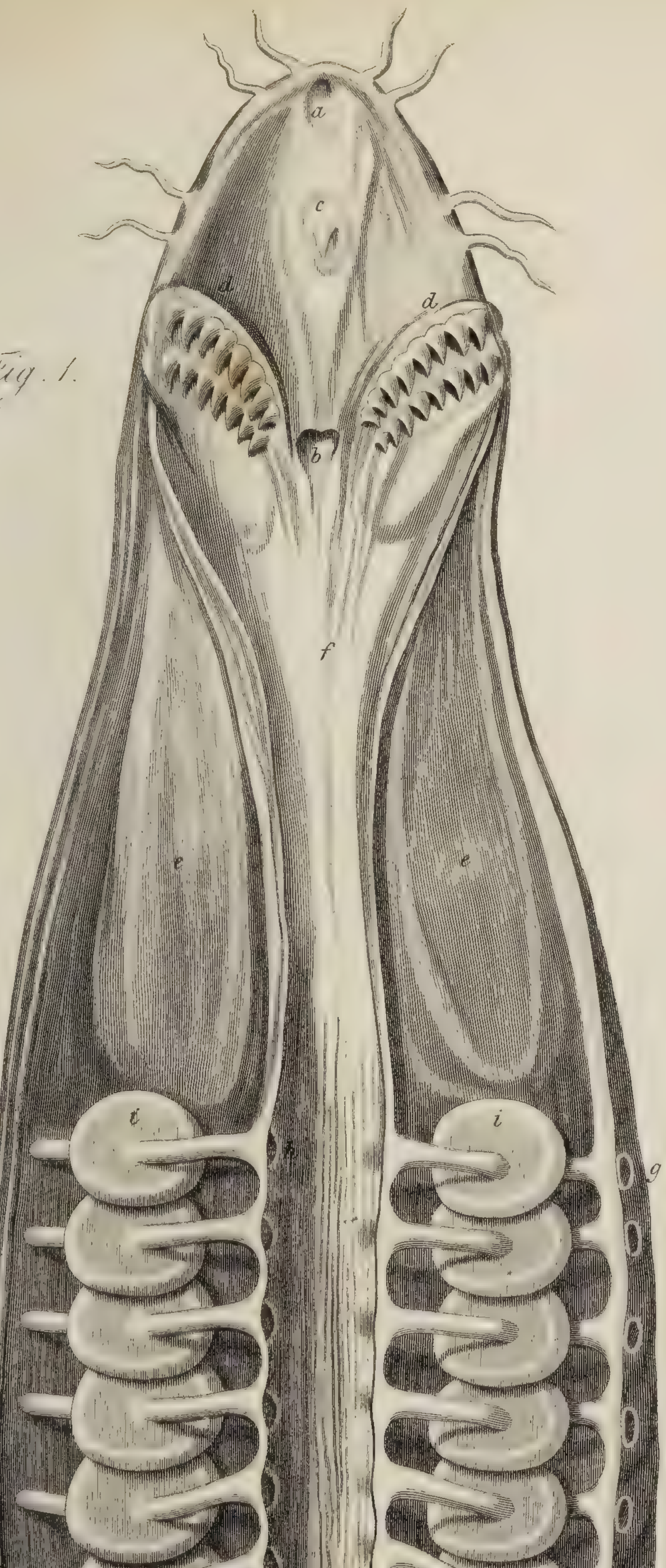
Fig. 1.



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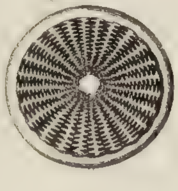

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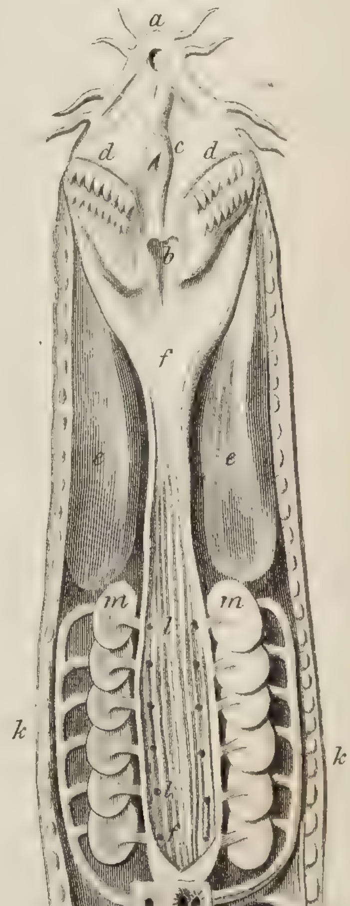

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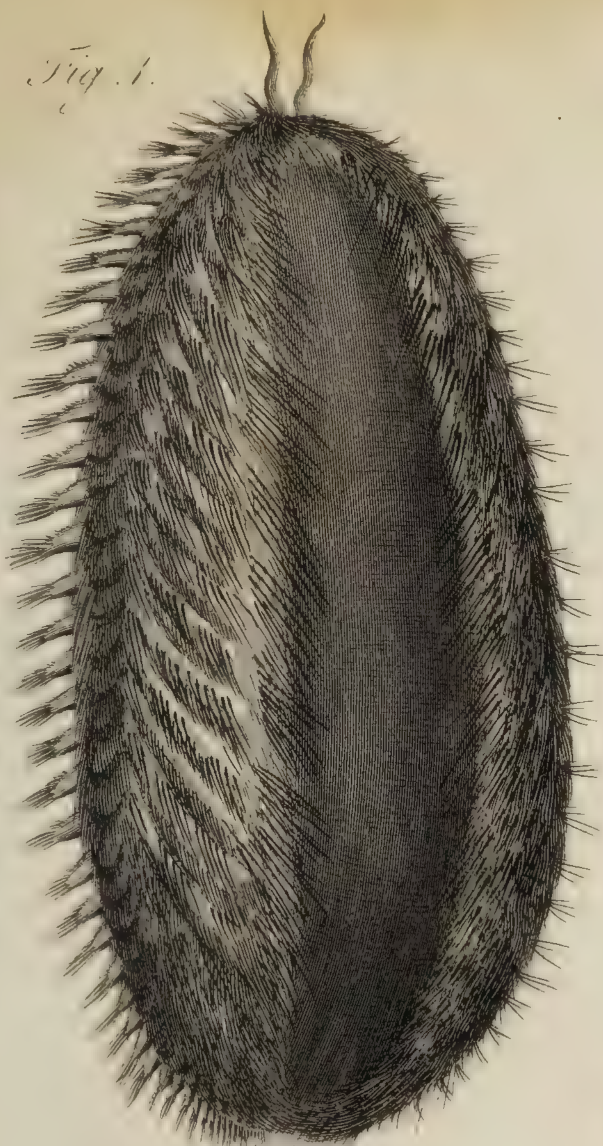

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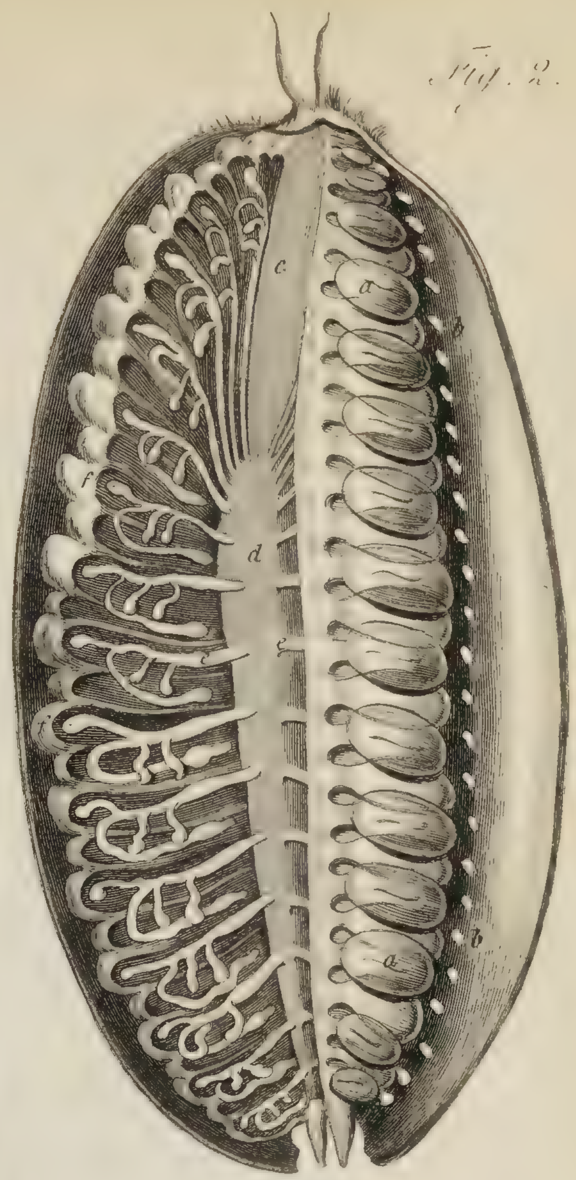

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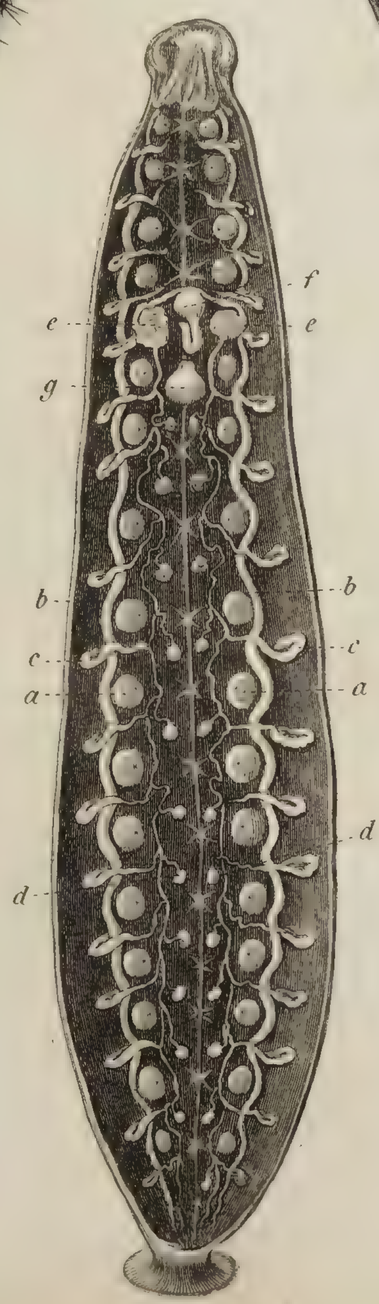


XVII. *On the mode of generation of the lamprey and myxine.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read June 15, 1815.

The observations contained in the preceding paper upon the organs of respiration of the lamprey and myxine, led me to doubt the propriety of classing these animals among fishes, and as their ovaria differ in many respects from those of fishes in general, I became desirous of knowing the structure of the testicles in the male, to see how far they resembled those of fishes, or in how great a degree they differed from them.

That the lamprey was male and female nobody seemed to doubt, and yet with every opportunity apparently before me, I was unable to procure one male. Sir Joseph Banks supplied me very largely with lampreys and lamperns, those that were full of ova were admitted to be females, and those which appeared to have no ova were said to be males; but all of them had ovaria, although in some the ova were extremely small, requiring a magnifying glass to distinguish them, in others they had been shed, leaving the cells in which they had been contained, and the openings by which they passed out very apparent. Being accidentally at Worcester, in which city, during the season, lampreys are caught in the river Severn in great abundance, and potted to be sent all over England, I made

inquiry of the person whose business it is to prepare them for being potted, what were the differences between the internal parts of the male and female ; he said, the only difference was, the one had no ova, the other had, in all other respects they were alike. He had never seen a lamprey in which there was no part corresponding to what I called ovarium. This remark from a person whose whole employment during the breeding season was to take out their viscera, corresponded so entirely with my own observations, that I began to entertain the opinion that the lamprey has not distinct sexes, but is an hermaphrodite animal. This doubt of their being male and female, was started in the beginning of the breeding season, and my friend Dr. WILSON PHILIP of Worcester, supplied me with lampreys at regular intervals, till the ova were shed, that I might prosecute this inquiry. I found upon examination, that the two glandular bodies projecting into the belly, one on each side of the ovarium, which have been always supposed to be the kidneys, varied very much in size and appearance at the beginning and end of the season. When the ova are so small that the animal is reputed to be a male, these glandular bodies and the black substance upon which they lie appear to form one mass, and the duct upon the anterior part is thin and almost transparent, containing a fluid equally so, but in the end of May, when the ova increase in size, these glandular bodies become larger, more turgid, and have a distinct line of separation between them and the black substance behind ; their structure is more developed, being evidently composed of tubuli running in a transverse direction, and the ducts leading from them are thicker in their coats and larger in size.

On the 5th of June, the ova were found to be of the full size, and a small transparent speck not before to be observed was seen in each; at this time the tubular structure had an increased breadth, and the duct going from it contained a ropy fluid, which when examined in the field of the microscope, was found to be composed of small globules in a transparent liquid.

On the 9th of June, neither the ova nor the tubular structure had undergone any change.

On the 11th, the ova were of the same size, but the slightest force detached them from the ovarium, the tubular structure had increased still more in size, the fluid in the ducts was thicker, more ropy, and when water was added to it in the field of the microscope, it coagulated, and what was before made up of globules, had now the appearance of flakes.

As these specimens had been kept two days, and had been carried 120 miles before they were examined, the appearance of the tubular part was seen under a disadvantage; but I was so fortunate on the 12th of June, as to receive from Sir Joseph Banks, the viscera of two lampreys caught in the river Thames, one of which had shed its ova, the other just ready to do so. In this last, the tubular structure from being in a more recent state was very distinct, and the difference in texture and appearance, between it and the black substance behind it, was more strongly marked. It is from this specimen that the annexed drawing was made, (Pl. XIV.) in which the black part is seen to have a reticulated texture. As it runs up as high as the heart, and may be said to lie principally behind the peritonæum, which is both the course and situation of the kidney in fishes, there can be no doubt that it performs the office of

that gland, while the tubular bodies which project into the cavity of the abdomen, and are increased to double their usual size at the time of shedding the eggs, must be considered as the testicles.

The ova in the lamprey do not pass out at an excretory duct as in fishes, but drop from the cells in the ovarium in which they were formed into the cavity of the abdomen, and escape by two small apertures at the lower part of that cavity into a tube common to them and to the semen, in which they are impregnated.

This mode of impregnation is so much more economical than that employed in fishes, that it explains the circumstance of the testicles being so small.

In the animal, intermediate between the lamprey and myxine, and in the myxine, the organs of generation have the same structure as in the lamprey.

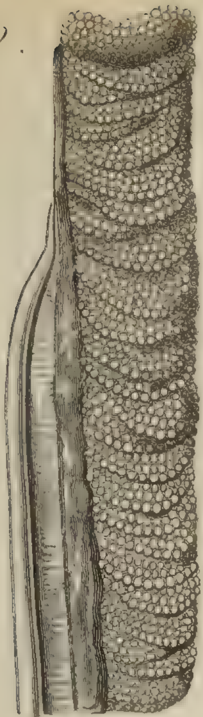
EXPLANATION OF PLATE XIV.

Fig. 1. A lamprey of the natural size laid open, to show the ovarium at the time the eggs begin to be shed, some of them lying loose in the cavity of the belly, others remaining in the cells of the ovarium in which they were formed.

On each side of the ovarium is seen a glandular body projecting into the cavity of the belly, which I have explained to be the testicle. It is made up of tubes placed in a transverse direction; behind it is a substance composed of a reticular

Fig. 1.

Fig. 2.



texture, which extends higher than the testicle; this I consider to be the kidney. On the forepart of the testicle is the duct or vas deferens; this is laid open at its termination to show that it forms, with the opening into the belly, a common cavity just within the verge of the anus.

Fig. 2. A portion of the ovarium and testicle of the natural size, when the ova are beginning to enlarge.

XVIII. *On the multiplication of images, and the colours which accompany them in some specimens of calcareous spar. By* David Brewster, *LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P.R.S.*

Read June 8, 1815.

Dear Sir,

The multiplication of images exhibited in some specimens of Iceland spar, appears to have been first observed by Dr. John Robison of Edinburgh, who showed the phenomenon to Mr. Benjamin Martin.* Having procured several specimens that had a similar property, Mr. Martin examined them with care, and published an account of his observations in his Essay on Iceland Crystal. The experiments of Martin were repeated by Mr. Brougham, who concluded that the images were produced by fractures, parallel or nearly so to the sides of the rhomboid, and Malus has more recently endeavoured to explain the phenomena, by the laws of extraordinary reflexion within doubly refracting crystals.†

All these philosophers agree in ascribing the multiplication of images to internal reflections, and they equally concur in regarding the colours of the images as the same with those of thin plates, and as produced by fissures, or fractures within the crystal.

* See Brougham's "Further Experiments, &c. on Light." Phil. Trans. 1797.

† I am acquainted with Malus's observations only from a short account of them given by Dr. Young.

In this state of the subject, my attention was accidentally directed to it, in consequence of having received, from Sir GEORGE MACKENZIE, Bart., some very fine specimens of calcareous spar, which possessed the property of multiplying and colouring the images. I examined with attention the different planes in which the images were polarised; I found that small crystals detached from particular parts of the specimens, possessed the same properties, and I represented in figures the specimens which I employed, and the interrupting planes by which the colours were obviously produced. These results convinced me, that the interrupting plane was not a fissure or fracture; and I conjectured that the colours were analogous to those produced by the action of crystals upon polarised light.* By following out this conjecture, I have been led to the true cause of all the phenomena, and of other analogous facts; and have thus been enabled to communicate to any specimen of Iceland spar, the faculty of multiplying and colouring the images, in a manner so exactly similar to the real specimens, that no person can discern the least difference between the phenomena of the artificial, and those of the natural crystal. The results to which this explanation leads, will, I trust, be equally interesting to the mineralogist and to the natural philosopher.

SECT. I. *On the phenomena exhibited by particular specimens of Iceland spar.*

Let AEBFHDGC, (Pl. XV.) fig. 1. be a rhomboid of calcareous spar, and let the supposed fissure by which the coloured images are produced, be in the plane ABCD. When a pencil of

* Treatise on new Philosophical Instruments, &c. p. 339, and Pref. p. xii.

light is transmitted through the faces BCGE and AFHD, or through the faces BFHC and ADGE, the object from which it proceeds has the appearance represented in Fig. 2., consisting apparently of three images. The middle image A is white and is composed of two images A, *b* polarised in an opposite manner like the double image formed by common rhomboids of calcareous spar. The image B, which is highly and uniformly coloured, is polarised like A, and the image *a* is coloured in the same manner as B, and polarised in an opposite manner like the other image at *b*.

Let the rhomboid be now placed in such a position, that a horizontal pencil of light is incident upon the vertical face BCGE, and let the rhomboid be turned round a vertical axis, so that the pencil may be incident at various angles, the plane of incidence being always parallel to the horizon.

When the angle of incidence is about 10°, and the ray inclined towards EG, the two images B, *a* vanish, their angular distance being then about $3\frac{1}{4}$°, but at every other angle of incidence, these two images are visible. When the angle of incidence is gradually diminished, till it vanishes and then increases on the other side of the perpendicular, the reappearing images B, *a* separate from the middle image. The image B separates from it more rapidly, and increases in magnitude, in the same manner as when a pencil of light is incident obliquely *towards the refracting angle* of a prism, while the image *a* separates slowly from A, and contracts its dimensions, as when a pencil of light is incident obliquely *towards the base* of a prism. When the angle of incidence increases from the position where B and *a* vanish, these images approach to the middle image

A*b*; constantly varying their colours in such a manner, that the middle image A*b* has *a colour complementary* to that of the extreme images B and *a*.

If the angle of incidence is made to vary in a vertical plane, the extreme images separate from the middle image when the pencil is inclined to G*c*, but they approach it, and exhibit the complementary colours when the inclination of the pencil is towards BE. During these changes the colour of A is often complementary to that of *b*.

When the eye is placed in front of the face BCGE, so as to perceive the images reflected from the posterior surface AFHD, each of the images *a*, *b*, A, B is tripled in passing the plane ABCD, so that *nine* highly coloured images are distinctly visible.

In a very curious specimen of calcareous spar in the possession of Mr. MYLNE, there are two supposed fissures as shown at *afhd* and *ebcg*, fig. 7. These planes are equidistant from the obtuse angles E, F, and each of them produces three images which are never coloured, except when the incident ray is very oblique, and the two extreme images near the middle one. In this case, the colour of the middle image is distinctly complementary to that of the extreme images. When the light passes through the faces AEGD, BFHC in such a manner, that the eye receives part of the pencil acted upon by the plane *e b c g*, and part of the pencil acted upon by the plane *a f h d* each of the three images appears double, and in consequence of a third plane $\varepsilon\beta\kappa\gamma$, one of the extreme images is sometimes tripled, so that the eye may see at once *seven* images independent of the numerous images

which are formed by reflections from the sides of the rhomboid.

All the specimens of interrupted calcareous spar which I have examined, present a very remarkable and beautiful phenomenon which has not hitherto been observed. Let AEBF, Fig. 3. be a section of the rhomboid shown in Fig. 7, and BA the interrupting plane; a ray RS incident in a direction nearly parallel to BA will be refracted in the direction ST, and passing through the plane BA at T, will emerge in the direction VX. Another ray *rs* incident on the adjacent face BF, and parallel to SR, will be refracted in the direction *s*T, and suffering reflection at *T* in the direction TV will also emerge in the line VX. If the face BF is covered, an eye placed at X will perceive across the middle image A*b*, Fig. 2. a series of beautiful fringes concave towards B, Fig. 3. and separating a bright from a dark space which is towards B. The predominant colour in these fringes is yellow: their direction is perpendicular to the line joining the images *a*, *B*, Fig. 2, and they increase in breadth towards E, Fig. 3. fading away in pink and green fringes. If the face BE is now covered, an eye at X will perceive a series of fringes complementary to the first set, and having a bright blue for their predominant colour. They have the same curvature and direction as the first set, and separate a bright from a dark space which is towards E, but they are far superior to them in distinctness and splendour of colouring. By covering only a part of EB, we can see at the same time both the sets of fringes, the yellow stripes of the first set joining the blue stripes of the second. This junction of the fringes forms a very interesting pheno-

menon, and is the only perfect example in which the complementary fringes are seen at the same instant.* In the specimen shown in Fig. 7, one of the interrupting planes gives remarkably minute fringes, while the other forms them of a larger size.†

SECT. II. *On the position and character of the interrupting plane.*

In every specimen of calcareous spar which possesses the property of multiplying and colouring the images, there is a plane ABCD, Fig. 1. stretching across the crystal. This plane, which I shall call the *interrupting stratum*, has not the most remote likeness to a fissure or fracture, but resembles rather a thin vein or film cohering to the two prisms between which it is interposed. The lines AB, CD which form the termination of the stratum, are distinctly marked on the natural faces of the crystal, and form straight lines perpendicular to the shorter diagonal FE; and the rhomboid is divided by the interrupting stratum into two equal prisms ABCDGE, ABCDHF, having the angles ABE, BAF each equal to 39°.

If the plane ABCD is a fissure or a stratum of air, as has been supposed, it is demonstrable that a ray of light incident at an angle of 37° upon AB will suffer total reflection, and therefore no light will be transmitted through the rhomboid. So far, however, from this being the case, there is actually no angle of incidence at which total reflection takes place at the second surface AB, and consequently there is no physical

* An imperfect example of this I have given in the Phil. Trans. 1814, p. 227. Plate VIII. fig. 3.

† In the specimens represented in Plate XI. fig. 8 and 11, of my Treatise on New Philosophical Instruments, the fringes are very large.

breach of continuity between the two prisms. If the adjacent surfaces of the prisms were perfectly smooth, and flat, like plates of parallel glass, and if they were pressed together by a great force, total reflection would thus be prevented, and the light would pass through the fissure at any obliquity. But if total reflection were prevented in this manner, the pencil of light would experience no peculiar action in passing through the compressed surfaces, and therefore neither a multiplication of images, nor a decomposition of the pencil into colours could take place.

With the view of corroborating this reasoning, I endeavoured to separate the two prisms by force, but I found this quite impracticable. The crystal actually broke at another place, so that the two prisms cohere with great force, though this is the direction of one of the cleavages of calcareous spar, and though the supposed fissure extends to the very surface of the four faces of the rhomboid.

But admitting the existence of a fissure under these circumstances, it is demonstrable that it could not produce the phenomena described in the preceding section. I have examined several real fissures in calcareous spar, and though the colours of thin plates were seen by reflection, yet those formed by transmitted light, could scarcely be rendered visible.

The appearance on the middle image of colours complementary to those on the extreme images, is an irrefragable proof that they are not the colours of thin plates, in which one of the complementary tints must necessarily suffer reflection.

In order to remove all doubt respecting the effects of a fissure, I ground off the angles EG, FH, Fig. 1. till the

crystal was bounded by the artificial faces *a f h d, e b c g*, and having polished these faces, I transmitted a pencil of light through the interrupting plane. In this case there was neither a multiplication of images, nor a production of colour, and the same result was obtained though I caused the pencil to fall upon the interrupting stratum at the same angle at which it was incident when the images were multiplied and coloured. Hence, it is obvious, that if the colours were produced by a fissure, they ought still to have appeared even when the fissure was bounded by parallel plates of spar.

In the specimen which is shown in Fig. 7, we are presented with several curious facts relative to the interrupting plane. This specimen is intersected by three interrupting strata *afhd*, *ebcg*, $\varepsilon\beta\kappa\gamma$, the two first being equidistant from AB, and all of them having the same position relative to the axis of the rhomboid. The thickness of the interrupting strata is distinctly seen at *af* and *eb*, and is nearly $\frac{1}{200}$ dth of an inch, bounded by two distinct parallel lines. The upper surface *af* of the stratum *afhd* is on a level with the general surface AEBF, but the upper surface *e b* of the other stratum forms an angle of 141° with the plane *e* E *b* and is smooth and well polished. The lower surface *dh* is partly level with the general surface, and partly inclined at an angle of 141° to the plane *d H h*, and the surface *gc* is parallel to the inclined surface *eb*. The two strata *afhd, ebcg* have therefore a crystallized structure, and as they effervesce with nitric acid, we are entitled to consider them as flat rhomboidal veins of calcareous spar.

The stratum *ebcg*, which is shown separately in Fig. 8, is divided into portions by four or five veins *mn*, *op*, some of

which, such as *r* and *s*, are not complete. When any of these minute veins, such as *mn*, is seen through the faces *e* E G *g*, *b* E G *c*, Fig. 9, it is quadrupled, and appears, as in the figure, composed of four veins *m* 1, *m* 2, *m* 3, *m* 4, and *n* 1, *n* 2, *n* 3, *n* 4 diverging from *m* and *n*. By gently inclining the rhomboid, all these veins are brilliantly coloured, and, what is very singular, the colours of the middle veins *m* 2, *m* 3, *n* 2, *n* 3, are always complementary to the colours of the extreme veins *m* 1, *m* 4, *n* 1, *n* 4, exhibiting a much greater variety of hues than is seen in any other position of the crystal.

In order to observe the connection between the stratum *eb* and the contiguous prisms, I cut off part of the prism *e* E *b* and laid bare the surface of the stratum towards E. I then removed the stratum itself till I came to the adjacent surface of the prism, and in both cases I found the particles of the prisms adhering firmly to the stratum, though they were at such a distance from it that light incident obliquely suffered reflection.

From these experiments, we may safely conclude, that the interrupting stratum is not a fissure or fracture ;—that it is a crystallized vein of calcareous spar, cohering firmly to the adjacent mass ;—and that the multiplication of images and the colours which accompany them, are produced only when this vein is interposed between two solid prisms.

Sect. III. *On the cause of the multiplication of the images.*

If a ray of light RS is incident upon a rhomboid EBFA, Fig. 4, interrupted by a stratum of air AB, it will be divided by refraction into two pencils S*a*, S*b*. These pencils will be again refracted at the second surface *m n* into the directions

ac, *bd*, and falling upon the second prism at *c*, *d*, each of them will be again divided into two pencils, viz. *ac* into the pencils *ce*, *cf*; and *bd* into the pencils *dh*, *dg*. The pencils *cf*, *dg* emerging parallel to each other, will form a double image like A*b*, Fig. 2, while the other two pencils *ce*, *dh* will be inclined to these, and will form the single images *a*, B. The images will therefore be multiplied exactly as in Fig. 2, and if we calculate their angular distance, we shall find it coincident with the experimental results. This multiplication of the images may perhaps be more easily comprehended by supposing A, B, Fig. 2. to be two images formed by the first prism ABE, Fig. 4; then as the second prism ABF has an equal refracting angle, but placed in an opposite direction, it will refract the image B to *b*, and the image A to *a*, thus forming a double image in the middle, and a single image on each side of it polarised in the manner described in Sect. I.

In the preceding reasoning it is assumed, that there is an interruption in the structure of the rhomboid by which a subdivision of the rays takes place within the crystal. We shall now enquire how such an effect can be produced without a fissure. If we divide a rhomboid into two prisms ABE, ABF, and fill up the interval AB with a cement of the same or of a different refractive power from that of the calcareous spar, the ray RS will emerge in four pencils *ce*, *cf*, *dg*, *dh*, just as when AB was a stratum of air, and in so far as the multiplication of images is concerned, this artificial rhomboid will exhibit the precise phenomena described in Sect. I.

Hence it follows, that the multiplication of images arises from a subdivision of the two pencils at the first surface

of the second prism, and that the great angular distance of the images, which takes place even when the prisms are connected by a cement in perfect contact with each, is occasioned by the action of the doubly refracting force near the second surface of the first prism.

As all the phenomena of the natural crystal may be imitated by an artificial one, there can be no doubt that such changes actually take place within the crystal. It is interesting, however, to ascertain the principles on which these changes depend; and we are fortunately able not only to do this, but to apply the principles to the explanation of other phenomena exhibited by doubly refracting crystals.

It may be shown by various experiments, that the division of a beam of light into two pencils by double refraction, does not take place till the light has penetrated the first surface of the crystal, and suffered the ordinary refraction, while at the second surface the extraordinary refraction takes place before the emergence of the ray. The interposition, therefore, of a film AB of the same refractive power as the crystal, though it prevents the ray from suffering any ordinary refraction, still allows the extraordinary refraction to take place just as if the prisms were completely separated. For the same reason, the extraordinary refraction again takes place at the first surface of the second prism, and the two pencils are divided into four, as represented in Fig. 2.

Since the prisms ABE, ABF, Fig. 4, or the rhomboids which they contain, have their homologous sides parallel, the pencils *Sa*, *Sb* ought not to be divided into two by the second prism according to the observations of Huygens and Newton.

This, however, is true only when the pencil is incident at an angle of between 12° and 14°, as described in Sect. I.,* and we have already seen that in this case the images are reduced to two. In every other position of the incident ray, the pencils are subdivided by the second prism.

The division of the pencil into two parts after it has penetrated the first surface, or before it has emerged from the second surface of calcareous spar, enables us to explain the curious fact observed by MALUS, relative to the light reflected from the interior surface of doubly refracting crystals. He discovered that the ray refracted ordinarily at the second surface was reflected at this surface in two pencils, one ordinary, and the other extraordinary; and that the ray refracted extraordinarily at the second surface, was also reflected in two pencils; so that there were four reflected rays, and only two emergent ones. These four rays returning to the first surface of the crystal, emerge in four parallel pencils, which form with this surface the same angle as the incident ray.

The cause of this singular fact will be understood from Fig. 5, where ABCD is a piece of calcareous spar, and mn, $\mu\nu$, the lines within the crystal at which the extraordinary refraction takes place. A ray of light RS will be divided into two pencils Sa, Sb, which will emerge in lines $a\,\alpha$, $b\,\beta$, parallel to RS. The reflected portions bd, ac will be subdivided at c and d, just as if they had been incident in the directions $\delta\,b$, $\kappa\,a$, and will form four pencils ce, cf, dh, dg, which is the phenomenon observed by MALUS. In order to show experimentally that the rays $a\,\alpha$, $b\,\beta$ are subdivided at c and d, when received upon the rhomboid in the directions $\delta\,b$, $\kappa\,a$, cement

* I shall have occasion to consider this law in a subsequent Paper. I have stated the angle at between 12° and 14° as the pencil dh Fig. 4. vanishes at a less angle than ce.

a plate of glass GH, Fig. 6. upon the second surface CD, by means of a transparent cement EF. The rays S*a*, S*b* have now emerged completely from the calcareous spar, and being reflected from the glass plate GH, they again enter the crystal, and are subdivided as formerly at the line *mn*, into the four pencils *ce*, *cf*, *dg*, *dh*.*

In a specimen of calcareous spar examined by Mr. Martin, *twelve* images were seen, arranged in three rows. The middle row, consisting of *six*, was produced by two interrupting planes situated in the manner shown in Fig. 7, while the other two rows were formed by reflection from the sides of the rhomboid. Mr. Brougham examined a specimen which afforded *six* images in some positions, besides other two, which, as this able writer justly remarks, were reflected from the sides of the specimen.

Sect. IV. *On the cause of the colours with which the images are affected.*

As there are some specimens of calcareous spar in which the multiplication of the images is not accompanied with the production of colours, the one phenomenon is not necessarily connected with the other, the multiplication of the images depending merely on the interruption in the regular structure of the mineral, and the colours upon the thickness and crystalline nature of the vein by which that interruption is produced.

We have already seen that the double image A*b* (Fig. 2.) is in general white, while *a*, and B, are affected with the same prismatic colour, and that when A*b* is coloured at particular angles of incidence, its colour is always complementary to

* Malus believed that the fact of the subdivision of the reflected pencils was general; but there is obviously a particular angle of reflection at which four pencils are not formed.

that of *a* and B. These colours are therefore produced by the transmission of polarised light through the crystallized film AB, Fig. 4. The light is first polarised by the prism ABE: it is then separated into its complementary colours by the crystallized film AB, and this compound beam is analyzed by the second prism ABF. This arrangement, indeed, is the very same as that which I have described in a former paper,* as necessary for the exhibition of the complementary colours, the light being polarised by double refraction instead of by reflection, and being analyzed by a prism of calcareous spar, instead of a plate of agate.

In order to put this explanation to the test of direct experiment, I cut a rhomboid into two prisms ABE, ABF, Fig. 4, having equal refracting angles; and I interposed a thin plate of sulphate of lime between the two prisms. When the light was incident on the first surface EB at an angle of between 12° and 14°, so that the two images *a*, B, had vanished, I shifted the sulphate of lime till it ceased to depolarise the light, or restore the vanished images *a* and B. I then cemented it in this position to the two prisms, and thus obtained an artificial rhomboid, which imitated with the utmost exactness all the phenomena of the natural one. The extreme images *a*, B, became coloured, while the double image A *b* remained white, and the colours varied by varying the inclination of the plate to the incident rays. The images *a* and B approached to, and receded from, the middle image as in the natural crystal, and at particular incidences the middle image exhibited colours complementary to those of the extreme images, and of the very same kind with those in the natural rhomboid.

* Phil. Trans. for 1814, p. 210.

When the position of the sulphate of lime is changed, the depolarisation is increased, and the double image A*b* is no longer white, but always displays the colours complementary to those of *a* and B. In particular positions of the sulphate, the middle images become white at an oblique incidence.

In the specimen of calcareous spar represented in Fig. 7, the colours are by no means brilliant, and they appear only when the incident ray falls obliquely upon the rhomboid, with an inclination towards the base of the first prism. The reason of this will appear from Fig. 4. When the ray is incident obliquely towards the base of the first prism, as *r*S, it is refracted in the direction S *m*, and passes through the interrupting stratum AB nearly at its least thickness; whereas when it is incident obliquely in the direction ϱ S, it is refracted into the line S*n*, and passes obliquely through the stratum at a thickness too great to produce the complementary colours. We are presented therefore with a method of determining rudely, the comparative thickness of the strata by which the colours are produced. In two very fine specimens, the colours are exhibited at almost every inclination, and they vanish when the inclination is near its maximum, and when the ray passes obliquely through the stratum.

Hence it follows, that the colours are produced by the transmission of polarised light through a crystallized vein, and that the phenomena change their character with the thickness of the vein.

We have already seen that the images may be multiplied without being coloured, but they cannot be coloured without being multiplied, as the separation of the oppositely polarised pencils is necessary to the production of the colours. This is

proved by grinding off the angles EG, FH. Fig. 1, so as to make the interrupting stratum parallel to the two faces of the crystal. There is in this case no multiplication of images, and no production of colour.

The complementary fringes described in Sect. I. are likewise produced by the transmission of polarised light through the interrupting stratum. When the ray RS, Fig. 3, is incident at various angles upon EB from the position *r* S, Fig. 4, to the position RS, Fig. 3, the refracted ray ST passes through the stratum AB at various thicknesses, and it therefore exhibits different colours corresponding to these thicknesses. When the ray has the position RS, the thickness of the film becomes so great, that the colours cease to be developed, and this limit of the production of colour is marked by parallel fringes gradually diminishing in breadth towards that limit. In like manner the ray *r s* being refracted in the direction *s* T, and falling upon AB at T, will pass through it at the same thickness as the ray RS does, and being reflected from the posterior surface of the stratum, will move in the direction TV, and emerge in the line XV. Fringes of the same character, but complementary to the former, will thus be produced by reflection, and from the equality of the angles STB, *s* TB and ATV, the fringes formed by transmission will be seen in the same direction XV as those seen by reflection. The reason is theref re manifest why the one set of fringes is seen by covering the face EB, and the other set by covering the face BF, and why both sets are visible when only part of EB is covered.

In order to show that these fringes are produced by the action of the crystallized film upon polarised light, I examined the phenomena in the following manner. As the double

image A b, Fig. 2, across which the fringes are visible, is formed by two equal and oppositely polarised pencils, it is necessary to the production of the fringes, that one of these pencils be either extinguished, or greatly diminished in its intensity. Now, if we cover BF, and examine with a prism of calcareous spar the pencil VX formed by the rays RS, we shall actually find that one of the images, b for example, Fig. 2, is very much fainter than the other, and therefore the fringes must be formed of the polarised light of A, being in this case very faint, owing to the admixture of the remaining light of b. If, on the contrary we cover EB, and examine the pencil XV formed by the rays rs, we shall find that the image b, Fig. 2, is almost wholly extinguished, and consequently the fringes formed by the polarised light of A are remarkably distinct, suffering no diminution of lustre from the admixture of oppositely polarised rays.

If the rays RS, rs, (Fig. 3.) are now polarised before their incidence upon the rhomboid, the fringes formed by rs, do not experience any change, in consequence of their being produced by unmixed polarised light; but the fringes formed by RS suffer a particular modification. When the plane of incidence EBFA forms an angle of 45° with the plane of polarisation, the fringes are extremely distinct and beautiful, and the same thing happens when the rhomboid is turned round 180°. In positions at right angles to these, no fringes whatever are visible. In the first of these positions, the pencil is not divided into two oppositely polarised pencils, whereas in the other position, it is divided into two oppositely polarised pencils of equal intensity.

If instead of polarising the incident rays RS, rs, we examine

the resulting pencil VX with a prism of calcareous spar, we shall find certain positions of the prism in which the fringes are invisible. In some positions they are finely displayed across the first image, and are not visible across the second; while in other positions the fringes across the first image vanish, and appear distinctly across the second. All these phenomena arise from the alternate evanescence of the images, which causes the fringes to be seen across the remaining image formed by light polarised in one plane.

The explanation which has now been given of the iridescent phenomena of calcareous spar, enables us to account for the origin of the colours peculiar to the agate, which I have described in a former Paper.* These colours always appear in veined agate, and are, undoubtedly, produced by the interposition of a vein between two equiangular prisms.

SECT. V. *Description of new instruments for exhibiting complementary colours.*

A simple instrument for exhibiting the opposite or complementary colours has long been a desideratum in the arts, as well as in the sciences. To painters, and to artists of almost every description, it is of very extensive use, while in many optical inquiries its advantages cannot be sufficiently appreciated.

The method of showing these colours which I have pointed out in a former Paper, consists in the separation of polarised light into two pencils, by the action of a crystallized plate, and in the subsequent analysis of the pencil, by a doubly refracting crystal. The simplest way of fitting up an instru-

* See Philosophical Transactions, 1813, p. 102, 103, and 1814. p. 197, 199.

ment upon this principle is shown in Fig. 10, where ABCD is a tube one or two inches long, S a piece of black glass forming an angle of 33° with the axis of the tube; *n* a convex eye glass placed next the eye; *o* an aperture of a circular or any other form in the focus of the lens *o; m* a flat piece of topaz or rock crystal* not much larger in diameter than the pupil of the eye, and cut in the proper direction from the crystal; BD a prism of nearly the same diameter formed out of rock crystal in the manner long ago described by M. Rochon,† so as to produce the greatest separation of the images, or what is still simpler, a prism of calcareous spar having the refraction and dispersion as much as possible corrected by an opposite prism of balsam of Tolu or indurated Canada balsam. When the instrument is thus fitted up, the rays RS, polarised by reflection from the glass S, are arranged into their complementary colours by the crystallized plate *m*, and are afterwards separated into two distinct pencils by the double refraction of the prism BD. An eye, therefore, placed at *n*, will see two distinct images of the aperture *o*, and the colour of the one image will be complementary to that of the other. These images will exhibit alternate variations of colour by turning round either the tube, or the polarising plane S. If

* A thin film of sulphate of lime is much better than any other mineral, as it requires no trouble to prepare it. Topaz is preferable to rock crystal, as the latter very often gives false tints, from a want of uniformity of structure. Mr. Sanderson, an ingenious lapidary in Edinburgh, has cut about twelve plates of rock crystal parallel to the axis of the pyramid, and observed, that all of them were filled with veins and imperfections radiating from the axis. In a large pyramid about $2\frac{1}{2}$ inches in diameter, these radiations are arranged in the form of a cross, forming angles of 60° and 120°, and they terminate on the faces of the pyramid.

† *Journal de Physique*, 1801. *Mémoire sur le Micrometre de Cristal de Roche.* Paris, 1807.

the two images overlap, the parts that overlap will be white, in consequence of the combination of the two opposite colours. The object of using the lens *n*, is to shorten the tube, but if we remove the eye glass, the aperture *o*, may be made of any size, and placed at any distance from the eye.

I have been induced to give this particular account of the preceding instrument, as another instrument upon the same principle, but of a most unphilosophical construction, has recently been exhibited in Edinburgh as a new invention, without any mention having been made either of M. Arago or myself, who separately discovered the property of polarised light on which it depends, or of M. Rochon who invented the eye-piece of the instrument. It consists of a tube from twelve to twenty inches in length for the purpose of producing a sufficient separation of the images, and a large object glass of rock crystal is placed at the very end of the tube, although a small piece $\frac{1}{6}$th of the size would have answered much better, and admitted of a larger aperture if placed near the eye. In order to polarise the light, the operator carries a large square plate of japanned metal, and places it as near the polarising angle as he can. The instrument is then directed to this plate, and exhibits two overlapping images affected with the complementary colours.

The investigation in the preceding pages, furnishes us with a principle for constructing a new *antichromatic instrument*, far superior to any of the preceding, and so very simple, that any person can make it for himself. It is represented in Fig. 11, where MNOP is a tube about two inches long attached to a ball and socket. The end MO of the tube carries an aperture of any form, and the ball CD contains two

prisms of calcareous spar separated by a film of sulphate of lime so placed that each pair of the four images is tinged with the complementary colours as described in Sect. IV. A lens L is cemented either upon the anterior or posterior surface of the compound prism, or may be kept separate from the prism at L, but whatever be its position, it must always enable the eye at E to see the aperture with perfect distinctness, and the focal length of the lens must be so adapted to the magnitude of the aperture, that the images of it can be sufficiently separated by the universal motion of the ball CD. The interior of the tube being covered with a black pigment, the instrument is ready for use. If we direct it to the sky, or to any luminons object, four brilliantly coloured images of the aperture will be distinctly seen, the colour of the two middle images being complementary to that of the two extreme images. By moving the ball in the socket, the colours will constantly change, and the images will sometimes over lap, and sometimes separate, exhibiting the finest variety of hues, and pleasing the eye by their combinations, and by the soft harmony of their contrasts.*

In the instrument where it is necessary to polarise the light by black glass, or japanned metal, there is no less than $\frac{12}{13}$ths of the incident pencil lost by reflection, while in the preceding instrument the light lost by transmission is very small. From this cause, the brightness of the colours is incomparably greater, and they may even be distinctly seen in candle light, by directing the aperture to a piece of white paper held near the candle.

* The phenomena will admit of many beautiful variations, by using several films of sulphate of lime, having their axes variously inclined to one another.

I shall now conclude this letter with the description of another instrument which I have found of great advantage in carrying on very delicate experiments on the polarisation of light. In comparing the quantities of light polarised in the plane of reflection by different metals, we derive very little aid from examining the partial evanescence of one of the images. The intensity of the complementary colours is a much more delicate measure of the portion that has received this character. The method of doing this is shown in Fig. 12, where ABCD is a tube about eight or nine inches long supported upon a stand. An equal and unbroken plate of *sulphate of lime* which gives an uniform tint in every part of its surface, is cemented with Canada balsam between two plates of parallel glass, and is placed at the end AB of the tube exposing two circular apertures *m*, *n*, to the incident rays. At the other end of the tube is a piece of black glass *op*, inclined at an angle of 33° to the axis of the tube, and having a motion of rotation round that axis. When polarised light is transmitted through the apertures *m*, *n*, and reflected from the surface *op* to the eye, these apertures will appear equally coloured in every position of *op*, the colour in one quadrant of its circular motion being complementary to that in the adjacent quadrants. If we now wish to compare the quantity of polarised light in a pencil reflected at an angle of 80° from *silver*, with the quantity polarised at the same angle by *steel*, we have only to transmit the one pencil through *m*, and the other through *n*, and the intensity of the colours will show which of the two contains the greatest quantity of polarised light. Or if we diminish the inclination of the steel surface

till the colours of the two apertures are equally intense, we obtain the angles of incidence at which *steel* and *silver* polarise equal quantities of light in the plane of reflection. By using two plates of black glass having a variable inclination to the axis of the tube, we may allow the light to fall at equal angles upon the two metals, and thus ascertain the different inclinations of the plates of glass at which the two apertures exhibit the same intensity of colour.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, May 1, 1815.

To the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. *&c. &c. &c.*

Fig. 1.

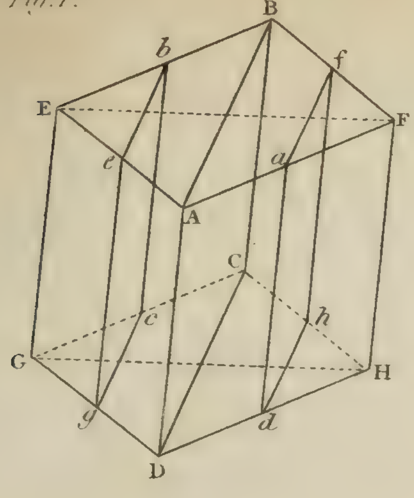


Fig. 3.

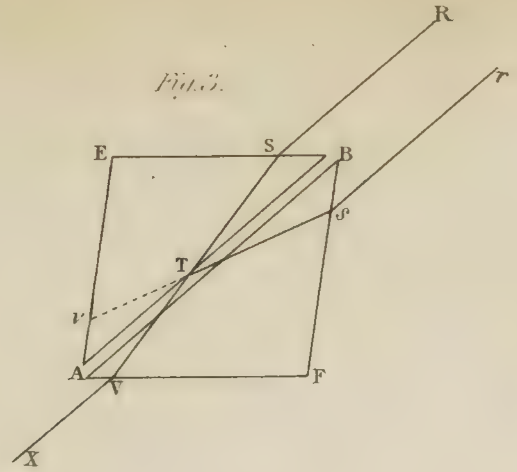


Fig. 4.

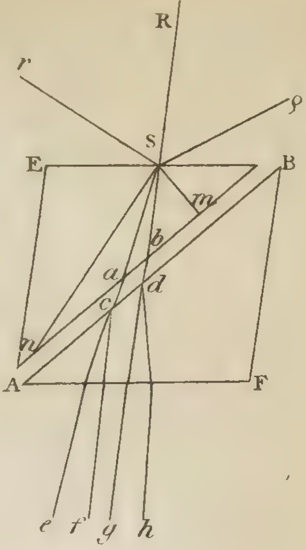


Fig. 2.

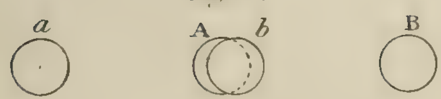


Fig. 5.

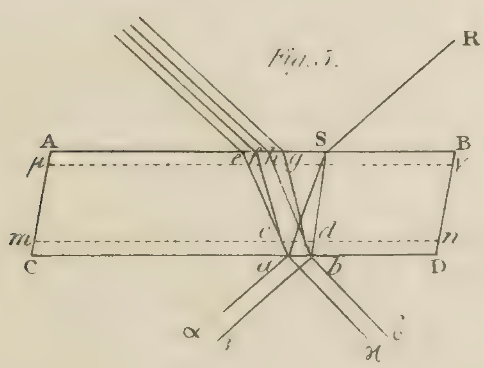


Fig. 6.

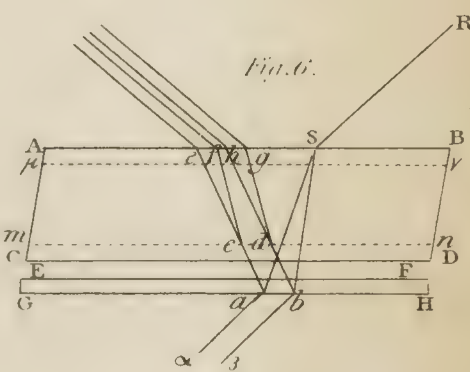


Fig. 7.

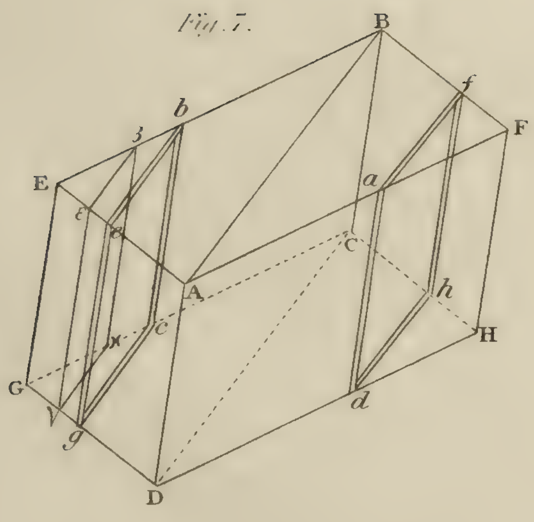


Fig. 8.

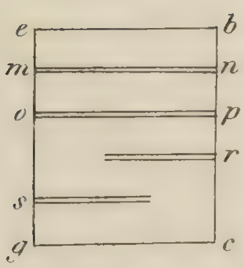


Fig. 9.

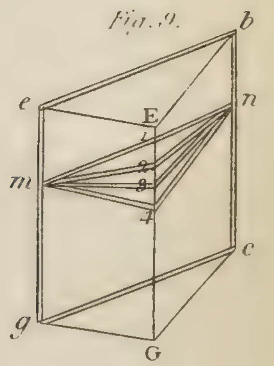


Fig. 10.

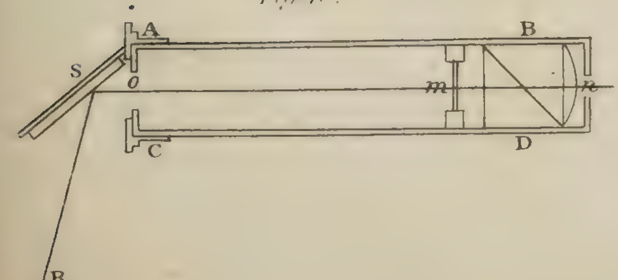


Fig. 11.

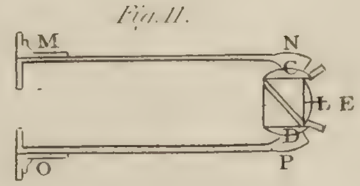
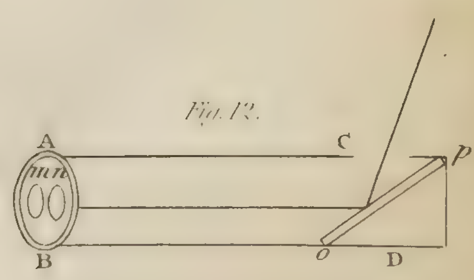


Fig. 12.



XIX. *A series of observations of the satellites of the Georgian planet, including a passage through the node of their orbits; with an introductory account of the telescopic apparatus that has been used on this occasion; and a final exposition of some calculated particulars deduced from the observations.* *By* William Herschel, *LL.D. F. R. S.*

Read June 8, 1815.

THE observations of the satellites of the Georgian planet, of which an account is given in this paper, are of such a nature that, in order to judge of them properly, and to make them useful to those who would continue them, it will be necessary to enter into some particulars relating to the telescopic powers required for critically viewing such difficult objects.

The great distance of the Georgian planet renders an attempt to investigate the movements of its satellites a very arduous undertaking; for their light, having to traverse a space of such vast extent before it can reach us, is so enfeebled, and their apparent diameter so diminished, that an instrument, to be prepared for viewing them, must be armed with the double power of magnifying and of penetrating into space.

With regard to the first of these requisites, I have already shown in a former Paper,* that the magnifying power of my ten feet telescope, when no uncommon degree of light is

* Phil. Trans. for 1805, page 31.

wanting, is fully equal to what may be required to view extremely small objects; but this branch of the properties of optical instruments seems not to be generally understood: the question how much a telescope magnifies, admits of various answers. To resolve it properly, we ought in all circumstances to consider how far the magnifying power of a telescope is supported by an adequate quantity of light; as without it, even the highest power and distinctness cannot be *efficient*. The question therefore ought to be limited to an inquiry into the extent of what may be called the *effective* magnifying power? It will however be found, that even then, the quantity of this power cannot be positively assigned. For if a card containing engraved letters of a certain size be put up at a given distance, the effective power of a telescope directed to it, will be that wherewith we can read these letters with the greatest facility; but if either the size of the letters, or their distance from the telescope, be changed, the quantity of this power will no longer remain the same.

An obvious consequence of this consideration is, that the effective power of telescopes has a considerable range of extent, and can only be assigned when the object to be viewed is given; and that in this determination two circumstances are concerned, which require a separate investigation; and this is abundantly confirmed when a ten feet reflector, such as has been mentioned, is directed to the Georgian planet; for with none of its highest powers can we possibly ascertain even the existence of the satellites.

Since, then, it is absolutely necessary that the power of magnifying should be accompanied with a sufficient quantity of light, to reach the satellites of this remote planet, it may

be useful to cast an eye upon the action of a power which is become so essential. Its advantages and its inconveniences must equally be objects of consideration.

A very material inconvenience is that mirrors, which must be large in order to grasp much light, must also be of a great focal length; and that in consequence of this, we must submit to be incumbered with a large apparatus, which will require an assistant at the clock and writing desk, and also an additional person to work the necessary movements. The machinery of my twenty feet telescope is however so complete, that I have been able to take up the planet at an early hour in the evening, and to continue the observations of its own motion, together with that of its satellites, for seven, eight, or nine hours successively.

The forty feet telescope having more light than the twenty feet, it ought to be explained why I have not always used it in these observations. Of two reasons that may be assigned, the first relates to the apparatus and the nature of the instrument. The preparations for observing with it take up much time, which in fine astronomical nights is too precious to be wasted in mechanical arrangements. The temperature of the air for observations that must not be interrupted, is often too changeable to use an instrument that will not easily accommodate itself to the change: and since this telescope, besides the assistant at the clock and writing desk, requires moreover the attendance of two workmen to execute the necessary movements, it cannot be convenient to have every thing prepared for occasional lucid intervals between flying clouds that may chance to occur; whereas in less than ten

minutes, the twenty feet telescope may be properly adjusted and directed so as to have the planet in the field of view.

In the next place I have to mention, that it has constantly been a rule with me, not to observe with a larger instrument, when a smaller would answer the intended purpose. To use a manageable apparatus saves not only time and trouble, but what is of greater consequence, a smaller instrument may comparatively be carried to a more perfect degree of action than a larger one; because a mirror of less weight and diameter may be composed of a metal which will reflect more light than that of a larger one; it will also accommodate itself sooner to a change of temperature; and when it contracts tarnish, it may with less trouble be repolished; to which may be added, that having two mirrors for the twenty feet always ready, my observations could never be interrupted by accidents which often happen to large mirrors, such as greatly injure, or even destroy their polish.

The quantity of light reflected by the mirror of a twenty feet telescope of my construction being known, and the satellites of the Georgian planet being the objects to be viewed, I may now examine the combined powers of this instrument, and assign the limits to which they may be stretched. It will however be proper first, to point out from experience some of the advantages that may be taken, if not to increase, at least not to obstruct, the penetrating power, by the full effect of which the magnifying power is to be supported.

The first precaution I ought to give is, that in these delicate observations, no double eye glass should be used, as it cannot be prudent to permit the waste of light at four surfaces,

when two will collect the rays to their proper focus. The hole through which they pass in coming to the eye should be much larger than the diameter of the optic pencils, and considerably nearer the glass than their focus; for the eye ought on no account to come into contact with the eye piece; and a little practice will soon enable the observer to keep his eye in the required situation. It is hardly necessary to add, that no hand should touch the eye piece.

With regard to the eye glasses, when merely the object of saving light is considered, I can say from experience, that concaves have greatly the advantage of convexes; and that they give also a much more distinct image than convex glasses.

This fact I established by repeated experiments about the year 1776, with a set of concave eye glasses I had prepared for the purpose, and which are still in my possession. The glasses, both double and plano-concaves, were alternately tried with convex lenses of an equal focus, and the result, for brightness and distinctness, was decidedly in favour of the concaves.

For the cause of the superior brightness and sharpness of the image which is given by these glasses, we must probably look to the circumstance of their not permitting the reflected rays to come to a focus.

Perhaps a certain mechanical effect, considerably injurious to clearness and distinctness, takes place at the focal crossing of the rays, which is admitted in convex lenses.*

* About the same time that the experiments on concave eye glasses were made, I tried also to investigate the cause of the inferiority of the convex ones; and it occurred to me, that an experiment might be made to ascertain whether the rays of light in

I have occasionally availed myself of the light of concave eye glasses, but a great objection against their constant use is, that none of the customary micrometers can be applied to them, since they do not permit the rays to form a focal image. Their very small field of view is also a considerable imperfection; in observations, however, that do not require a very extensive field, such as double stars or the satellites of Saturn and the Georgian planet, this inconvenience is not so material.*

As I have already shown that the *effective* power of a telescope arises from the combination of its magnifying and space penetrating powers; and have also proved that the effect of their union, when they are differently combined, must have a considerable range, it will now be easy to point out the extent of this range in the telescope by which the following observations have been made.

The magnifying power by which the satellites of the planet were discovered was only 157; but this power, which has been

crossing, jostled against each other, or were turned aside from their right lined course by inflections or deflections. With a view to this, I directed a 10 feet telescope to some finely engraved letters put up at a convenient distance. A convex eye glass was fixed to a skeleton apparatus, which left the focal point freely exposed. A concave mirror was placed so as to throw the focus of the sun's rays upon the focal image of the telescope, where, meeting with no intercepting body, they would freely pass through it at right angles. Then a screen being placed to keep off the solar rays, I fixed my attention upon the letters viewed in the telescope, and the screen being alternately withdrawn and replaced, I could perceive no sensible alteration in the brightness or distinctness of the letters. Hence I surmised, that the rays of light did not sensibly jostle in an instantaneous right angled passage, but that possibly they might suffer inflections or deflections in their crossing at the focal point on account of their being longer in collateral proximity.

* See Phil. Trans. for 1794, p. 58.

constantly used in my sweeps of the heavens, and was found to be very *effective* for the discovery of faint nebulæ and minute clusters of stars, is hardly sufficient to show the satellites steadily; for, unless every thing is favourable, their faint scintillation will only be perceived by interrupted glimpses.

The magnifiers 300, 460, 600 and 800, it will appear by the following observations, have gradually been found to be more effective on the objects on which they were used; according to the clearness of the air, the altitude of the planet, the absence of the moon, the high polish of the mirror, and other circumstances: on particular occasions, when doubtful points were to be resolved, even 1200 has been most effective. The higher magnifiers 2400, 3600 and 7200 have also been used to scrutinize the closest neighbourhood of the planet, in order to discover additional satellites; but, from the appearance of the known ones, which began to be nebulous, I concluded that these powers were not distinct enough to be used on this occasion.

As the following observations are given for the purpose of enabling astronomers to calculate the elements of the orbits and motions of the satellites with mathematical precision, I have endeavoured to save them some labour by giving a clear statement of the general outlines of them; and that some judgment may be formed of their accuracy, which I hope will be found considerable, a short detail of the method I have pursued will be necessary.

For ascertaining the position of the satellites from which their periodical revolutions were determined, three different methods have been used.

Coarse estimations were made when they seemed to be

sufficient to keep the satellites in view, by way of ascertaining their identity; for unless they were followed in their course and known to be satellites, it would have been endless to measure either the distance or position of every small star that might have the appearance of one; and as the opportunities for taking measures, which require a very clear and undisturbed atmosphere, were scarce, and often interrupted by cloudy or moonlight nights, the identity of the satellites would have been doubtful if their position had not been attended to, when seen in unfavourable circumstances. When no other stars interfered, it was often sufficient barely to mention the quadrant in which they were seen, by recording that such a satellite was np, nf, sf, or sp; or if necessary, some rather more determined account, such as 40 or 50 degrees np, sf, &c.

As a check upon the description of the situations, a figure was always added to represent the planet, its satellites and the neighbouring stars as they appeared in the telescope. Very often indeed the configuration itself was deemed to be sufficient to point out the situation of the satellites, which by way of distinction were marked by numbered points; 1 and 2 being used to distinguish the known satellites; 3, 4, 5, &c. those that might possibly be other suspected, but not ascertained ones. Stars instead of points, were marked by asterisks.

More careful estimations were made with a power not less than 300, and a wire in the focus of the eye glass, to ascertain the parallel; they are capable of considerable accuracy in situations that are only a few degrees north or south preceding or following, and also when the position of a satellite is nearly 90 degrees north or south of the planet.

Measures taken with the micrometer may always be supposed to be accurate, unless they are marked as being affected by some circumstances existing at the time they were taken: when these are favourable, they can hardly be liable to any great error.

The calculations which I have given with the observations, will show the appropriate confidence each of these three methods of obtaining the positions of the satellites may separately deserve.

A much greater difficulty attaches to taking measures of distances than to those of angular positions: when the latter are taken, we have the position of the satellite in view all the time the planet passes along the parallel; and, although the moment of ascertaining the angle is only that in which the planet is in the centre of the wires, yet a constant attention to the motion of the two bodies will sufficiently enable us to perceive any excess or defect in the parallelism between their situation and that of the adjustable wire, whereas in measures of distances, the telescope must be kept in motion to retain the two bodies in their contact with the two wires, which disturbance considerably affects the delicacy of vision, and moreover requires a divided attention, as the passage of each body over its respective wire must be viewed. The only exception is, when the satellite is at 90 degrees, in which case the distance of the two bodies may indeed be measured with great accuracy.

The lucid point micrometer which has been tried is subject to the same difficulties;* its application to my construction of the 20 feet telescope, with regard to situation, is very conve-

* For a description of this micrometer, see Phil. Trans. for 1782, page 163.

nient. When the apparatus was preparing, I found that handles, 20 feet long, would be very cumbersome, and attempted to try the micrometer with the assistance of a person to arrange the points; but, when engaged in the first measure, I found that unless I had myself the command of the motions, a perfect adjustment could not be obtained; or would at least take up so much time as would bring on an alteration in the telescopic motions, not consistent with perfect vision. This micrometer has, however, the peculiar advantage that it may be used with a concave eye glass.

When a satellite is either directly preceding or following the planet, its distance may be measured by the difference of the time of their passing the meridional wire. This method, which has also been tried, is however not sufficiently delicate for very small intervals, and is moreover of little use, on account of the very limited situations.

The following observations on the satellites of the Georgian planet are given in the order of time they were made. They contain every thing that relates to the appearance and motion not only of the two principal large satellites, that are plainly within the reach of a 20 feet telescope of my construction, but also the more difficult researches that have been pursued for detecting additional satellites. That such there are I can have no doubt; but to determine their number and situation will probably require an increase of the illuminating power, such as I was in hopes, when I published my announce of their existence, would have been used by other astronomers, in pursuit of the subject pointed out to them; a 25 feet reflector which is mentioned in the observations, may probably be sufficient for the purpose.

To facilitate calculation, the observations are all given in mean time, and after each of them is added a theoretical exposition of the place of the satellites, which I have called an identification, and is denoted by the sign ‡; the great use of which will be to point out the validity of each observation, by comparing the observed places with the theoretical ones. The method of identification, which will be described hereafter, by giving not only the angle of position at which a satellite ought to have been seen, but also its proportional distance in 600 dth part of the radius of its orbit, is of great consequence when the orbits of the satellites are much contracted. These distances indeed become at last the only criterion by which we may know the satellites, for the angle of position, when the planet is near the node of the orbits, admits of so little change that it ceases to be a direction for identifying them.

The same distance will also give us the total value of the measure of any distances taken by the micrometer, so far at least as to show which of them may be the most proper to be chosen for a more rigorous investigation.

An identification of supposed satellites cannot be made by calculation; but the observations of following and also of preceding nights, accompanied by accurate configurations, may ascertain whether the object in question be of a sidereal or planetary nature. For if by the removal of the planet a supposed satellite be left in its former place, it is decidedly a star; whereas a well ascertained absence from the observed place will make its planetary nature highly probable. Then also, if a configuration and description of every small star, that is situated in, and very near the path of the planet, has been previously made, and additional stars are afterwards

found to be near the planet, which cannot be accounted for, it becomes again probable that such questionable objects are of a planetary nature. And this being a kind of identification, I have added it after the calculated one, to every observation of doubtful objects, except where a supposed satellite is pointed out which there is reason to believe may be a real one; for in that case, the observations relating to the object in question, are given in their regular order.

It will not be necessary to give the configurations that were made at the time of observation; they generally contained the planet, its satellites, and some of the neighbouring stars, especially those that were in the path of the planet's motion; nor will it be necessary to mention lines and descriptions of situations of stars pointed out by letters affixed to them, as the observations are generally so redundant, that I found it highly necessary to compress them.

Observations of the satellites of the Georgian planet, accompanied by a theoretical determination of their situation, whereby their identity may be ascertained.

1787, January 11^d 12^h 13^m. There is a supposed first satellite about 42 or 43 degrees south following the planet; and a second about 45 degrees north preceding. A third supposed satellite is south following the planet.

‡ By the identifying method, it appears that a real satellite, called the first, was visible at the time mentioned about $45\frac{1}{2}$ degrees south following; which agrees with the estimation of the angle of its situation, and also with a configuration of the stars and planet, drawn at the time of observation. By

the same method it appears that a real satellite called the second, was visible about 65 degrees north preceding the planet; which situation agrees with the configuration and also sufficiently well with a coarse estimation, which, as there was no wire for the parallel in the focus of the eye glass, could not be accurate. The supposed third satellite, by subsequent observations, was found to be a star.

1787, January 12. The first and second satellites are not to be seen in the place where I saw them last night. The supposed third is left where it was. I can see no small star near the planet, but the evening is not sufficiently clear.

1787, January 14, $12^h\ 3'$. There are again three supposed satellites; I have marked them 1st, 2d, and 3d, without any particular reason for that order.

‡ The first satellite was 88° nf; which agrees with the situation of that which in the configuration was marked 1st. The second satellite was $22\frac{1}{4}$ sp, and agrees with that which in the configuration was marked 3d. In the configuration the numbers are placed according to their distances from the planet, and that which is marked 2d was found to be a star remaining in its place.

1787, January 17, $11^h\ 51'$. There are now again three supposed satellites. The first is south preceding the planet, and makes a right angle with the 2d and 3d. The second is at the angular point and is south of the planet but a little preceding. The 3d is north following the planet. I have also added a 4th and 5th. The night is very fine and my telescope bears a high power.

‡ The first was 34° sp, which agrees with the configuration. The second was south of the planet but a little following,

namely $80\frac{3}{4}$° sf; which agrees sufficiently well with an estimation made without a direction for the parallel. The supposed 3d, 4th, and 5th, by next night's observation, remained in their places as small stars.

1787, January 18, 11^h 45′. There are two supposed satellites; the first is directly south of the planet; the second is about 45 degrees south following, and a little farther from the planet than the first. With 480 the first is about 4 diameters of the planet distant from it; the second is about $4\frac{1}{2}$ or 5 diameters from the planet; the first is from the second about $2\frac{1}{2}$ diameters of the planet. There is no small star in the path of the planet that might be taken for a satellite to morrow.

‡ The first was $76\frac{2}{3}$ sf. The second was $59\frac{1}{3}$ sf; both these positions agree sufficiently well with the delineated configuration.

1787, January 24, 11^h 23′. The first and second satellites of January 18, are no longer in the place in which they were that night. There are two satellites; the first is about 45° np the planet; the second is about 80° np; it is brighter than the first. I had a glimpse of a 3d and 4th.

‡ The first satellite was 49° np; the second was $75\frac{1}{4}$ np, which agrees well with the estimations and with the configuration. The observations of the third and fourth were lost, the planet not being seen again till eight days after, when it would have taken up too much time to look for them.

1787, February 4, 6^h 21′. The first satellite is about 80° sp; the second is about 30° nf. There is too much day-light to see the satellites well. A third supposed satellite is south preceding the first; it is extremely small. There is but one

single small star in the path of the planet which to morrow night may be taken for a satellite.

‡ The first satellite was 50° sp; the second was 40° nf. This differs considerably from the estimations, probably owing to the remaining day-light; the satellites however could not be mistaken, as there were no other stars near the planet.

1787, February 5, 9^h 3′. Both satellites are certainly absent from the place where I saw them last night. The first is about 85° sf; the second satellite (miscalled a small star) is by the configuration at a great angle nf. The small star in the path of the planet observed last night remains in its place.

‡ The first satellite was 89° sf; the second was 69° nf.

1787, February 6. I compared the configurations of January 11, 14, 17, 18, 24, February 4 and 5 together, and found that, admitting one of the satellites to make a revolution round the planet in about $8\frac{3}{4}$ days, and supposing its orbit to be very open to the visual ray, there was always one that would answer to a projection made on that scale.

1787, February 7, 6^h 54′. A satellite (miscalled the third) is a few degrees south following 6^h 30′, another (miscalled the first) is about 65° np. A small star (miscalled the second satellite) is about 60° sp.

‡ The first satellite was $11\frac{1}{4}$ sf. The second was $68\frac{2}{3}$ np. Two days after, the miscalled second, was seen remaining in its observed place. In the course of about nine hours of observation, I saw the planet accompanied by its two satellites, very evidently moving together in the path of the planet.

1787, February 9, 10^h 39′. Both satellites are gone from the place where I saw them the 7th of February. The first

satellite (miscalled the second) is directly north of the planet; the second (miscalled the first) is a few degrees np.

‡ The first was $81\frac{2}{3}$° nf; the second 11° np.

1787, February 10, 8^h 57′. The first satellite is about 53° np. 8^h 33′. The second satellite is about 20° sp; a supposed third is about 45° sf. In a little more than four hours, I saw the satellites go on with the planet, and also in their orbits.

‡ The first satellite was $67\frac{3}{4}$° np; the second was 20° sp. The supposed third was lost, no subsequent observation having been made of it. Before I began observing, I had delineated their places on paper, on a supposition that one of them moved at the rate of $8\frac{3}{4}$, the other at that of $13\frac{1}{2}$ days the revolution.

1787, February 11, 13^h 28′. Between flying clouds I saw the second satellite.

‡ The satellite was $57\frac{1}{2}$° sp, which agrees with the configuration.

1787, February 13, 10^h 0′. The first satellite, with 300, is about 75 or 80° sp; its distance from the planet is about $\frac{3}{4}$ of a minute. The second is about 85° sf; its distance is one full minute; the estimations are by the field of view of the sweeping piece. Third, fourth, and fifth supposed satellites were marked.

‡ The first satellite was $68\frac{3}{4}$° sp; its distance was 553, the radius of its orbit being 600. The second was $80\frac{1}{2}$° sf; its distance was 599, the radius of its orbit being also 600. The third, fourth, and fifth supposed satellites proved to be stars. No great accuracy can be expected from the estimated distances given in the observation, the field of the eye piece, which took in 15 minutes, being much too large for the purpose.

1787, February 16, 9^h 38′. The two satellites are in the places where I had drawn them on paper. With a power of 300, and a wire for the parallel in the focus of the eye glass, the first satellite is, by very accurate estimation, about 5 degrees north following; at the same time, and with the same power and accuracy, the second is about 3 degrees south following. A third supposed satellite is pointed out.

‡ The position being so near the parallel, and by calculation also near the conjugate axis of the elliptical projection of the orbits, and therefore less liable to an error arising from the application of correction, have been fixed upon as standards for the calculation of the periodical revolutions of the satellites. The supposed third was next evening observed to remain in its place.

1787, February 17, 7^h 58′. I tried to measure the distance of the second satellite from the planet by a lamp micrometer. The lucid points were 246,4 inches from the eye, and when they were 14,4 inches from each other, I found that the adjustment of the distance, and angle of position could not be made to my satisfaction by an assistant, and gave up the measure. The magnifying power being 157, the opening of the points gives the angular distance 1′ 17″; but the measure when given up was still much too large.

‡ The satellite was $28\frac{1}{2}°$ nf, and the distance 505.

1787, February 19, 7^h 55′. Having delineated the situation of the satellites on paper, I found them in the expected situation. A third and fourth were added in the configuration.

‡ The first satellite was 58° np; the second was 82° nf. The supposed third and fourth proved to be stars.

1787, February 22, 7^h 14′. By the configuration the first

satellite is at a considerable angle sp; the second is at a moderate angle np. Third, fourth, and fifth satellites were noticed.

‡ The first was 76° sp; the second was 31° np. A long interval happening to prevent subsequent observations, the supposed satellites were lost.

1787, March 5, 7^h 14′. The first satellite is about 6° sf. 7^h 17′, the second is about 87° nf; a third is about 40° nf.

‡ The first satellite was $20\frac{1}{4}$° sf; the second was 87° np; the third proved to be a star. The planet was only observed about 3 or 4′, and it does not appear that great accuracy in the estimations was attempted.

1787, March 7, 7^h 12′. The first satellite is 82° np. 7^h 13′ the second is about 30° np. Very coarsely estimated. A third is about 6° nf; it seems to have a fourth close to it. Having some doubts about the fourth, I viewed it with 600 and 800; I saw it also well with 1200, and had a glimpse of it with 2400. These high magnifiers require a fine apparatus for adjusting the focus.

‡ The first satellite was $78\frac{3}{4}$° nf; the second was 41° np; the third and fourth proved to be stars.

1787, March 8, 8^h 52′. Both satellites were seen for a few minutes.

‡ The first was 70° np; the second was $9\frac{1}{2}$° np.

1787, March 11. I found that some friends who came to view the satellites saw them best with 480, when the planet was drawn to the margin just out of the field.

1787, March 15, 8^h 7′. The first satellite is about 48° nf; the second is 5° sf. The second satellite being so nearly following the planet, I tried to measure its distance by sidereal

time. Of eight transits, four gave 3″; three gave 3″,5; and one gave 4″; a mean of them is 3″,31; and the declination of the planet being 21° 57′ north, we have the apparent distance 45″,99; but I do not trust much to measures by time, in the manner these were taken without a system of wires in the focus of the eye glass, and with the clock and assistant at a considerable distance.

‡ The first satellite was 46° nf; the second was $2\frac{1}{4}$° sf, and its distance from the planet was 481; this would give the greatest elongation 57″,36 which is probably much too large.

1787, March 18, 8^h 3′. The satellites are in the place where I expected them. The first is 5 or 6° np; the second is about 75° nf; it seems to be farther from the planet than when it was near the parallel. I attempted to measure its distance by the parallel wire micrometer; eclipsing the satellite with one wire, and bisecting the planet with the other. The measure gave the distance 46″,46.

‡ The first was 21° np; the second was $83\frac{1}{2}$° nf; and its distance was 588; which gives the greatest elongation 47″,41.

1787, March 19. Both the satellites are in their expected situation, which for the first is 36° sp; for the second 79° np. At 7^h 48′ I took a good measure of the distance of the second satellite; it gave 44″,24. I attempted a second measure, but was interrupted before I had quite finished it to my liking; it gave 45″,98.

‡ The first satellite was $29\frac{3}{4}$° sp; the second was $74\frac{1}{3}$° np. and its distance 596. The expected situations, though calculated from imperfect tables, were sufficient to show that the satellites were not mistaken.

1787, March 20, 7^h 44′. I took three measures of the

distance of the second satellite; the first gave 40″,23: the second, with the remark, pretty full measure, gave 41″,89; the third with the addition, not too large, gave 40″,20.

‡ The satellite wa $52\frac{2}{3}$° np, and the distance 564.

1787, April 9, 10^h 22′. I took two very accurate measures of the distance of the second satellite from the planet; the first gave 44″,54, the second 44″,35. By temporary tables its expected place was 57° sf.

‡ The satellite was 54° sf; and its distance 563.

1787, April 11, 9^h 18′. By temporary tables the expected situation of the second satellite was 4° sf. I took three good measures of its distance from the planet; the first gave 34″,47; the second 35″32; the third 35″,74. A mean of them is 34,″99.

‡ The satellite was $1\frac{1}{2}$° nf; and its distance 477.

1787, April 17, 8^h 53′. The two satellites are on opposite sides of the planet.

‡ The first was $40\frac{1}{2}$° sf; distance 531. 9^h 6′, the second was $21\frac{2}{3}$° np; distance 503.

1787, September 19, 15^h 55′. The first satellite (miscalled second) is 85° sp; the second (miscalled first) is about 30° sf.

‡ The first was $87\frac{3}{4}$° sp; the second was $10\frac{3}{4}$° sf. This being the first time of seeing the planet after its conjunction, accounts for the mistakes of the names.

1787, October 11, 16^h 49′. The first satellite (miscalled second) is 78° np. Two good measures of its distance from the planet were taken; the first gave 35″,18, the second 35″,96; a mean is 35″,57, 16^h 51′. The second satellite (miscalled first) is 40° sp.

‡ The first was 84° np, and its distance 599; the second was $52\frac{2}{3}$° sp; and its distance was 492. The long interruption in the observations was again the cause of a mistake of the names, which the calculation sets right.

1787, October 14, 15^h 59′. The angle of position of the first satellite by the micrometer is 48° 22′ sp; that of the second at 16^h 29′ is 66° 2′ sf.

‡ The first satellite was $49\frac{1}{2}$° sp; the second was $65\frac{1}{2}$° sf.

1787, October 20, 15^h 36′. Position of the first satellite by the micrometer 72° 0′ np. Position of the second at 16^h 8′, 80° 12′ np.

‡ The first was $76\frac{2}{3}$° np; the second was $80\frac{1}{2}$° np.

1787, November 9, 15^h 56. The second satellite is about 87° sf. The distance by four good measures 46″,15; 43″,92; 42″,94; 46″,57; mean 44″,89.

‡ The satellite was 84° sf; distance 594.

1788, January 14, 12^h 3′. The two satellites are almost in opposition; but the first precedes a line continued from the second through the planet.

‡ The first satellite was $77\frac{1}{4}$° nf; the second was 66° sp.

1789, February 22, 9^h 48′. The first satellite is about 80° sp; the second is about 85° sp; too much wind for measuring.

‡ The first satellite was $69\frac{1}{2}$° sp; the second was $85\frac{1}{3}$° sp.

1789, February 24, 9^h 13′. The first satellite is a few degrees more advanced in its orbit than the second.

‡ The first satellite was $48\frac{1}{3}$° sf; the second was 55° sf.

1789, March 13, 9^h 1′. The first satellite is 60° sf. 7^h 47′, the second is about 45° nf, third and fourth satellites were marked.

‡ The first satellite was $63\frac{1}{3}°$ sf; the second was $57\frac{2}{3}°$ nf. The third and fourth were found to be stars.

1789, March 14, 9^h 22′. The first satellite is 8° sf; the second is 70° nf.

‡ The first satellite was $19\frac{1}{4}°$ sf; the second was $81\frac{1}{4}°$ nf.

1789, March 16, 7^h 33′. The first satellite is 83° nf; the second is about 60° np. A third, about 2° sf; a fourth, about 8 or 10° np.

‡ The first satellite was $73\frac{1}{3}°$ nf; the second was 61° np. The third and fourth were stars.

1789, March 20, 7^h 50′. The two satellites were coarsely estimated to be at considerable angles sp.

‡ The first satellite was 63° sp; the second was $65\frac{1}{3}°$ sp.

1789, March 26, 10^h 44′. A star was mistaken for the first satellite; the second satellite (miscalled the first) is 45° nf.

‡ The first satellite was $63\frac{1}{4}°$ np; the second was 50° nf.

1789, December 15, 10^h 54′. The first satellite is about 71° sp. 10^h 49′, the second is about 75° sp; a third is about 75° sf.

‡ The first satellite was 72° sp; the second was $81\frac{1}{3}°$ sp; the third was a star.

1789, December 16, 10^h 12′. The first satellite is about 83 or 84° sf; the second is 85° sf. By the configuration they are very nearly in conjunction.

‡ The first was $83\frac{2}{3}°$ sf; the second was 83° sf.

1790, January 18, 9^h 32′. The first and second satellites are in the places I had calculated. There is a supposed third satellite about two diameters of the planet following, ex-

tremely faint and only seen by glimpses; $1^h\ 6'$ after I could not perceive it; a fourth is about 70° np.

‡ The first was $38\frac{3}{4}$° sp; the second $85\frac{1}{4}$° nf.

1790, January 19, $9^h\ 34'$. There is a very small star left in the place where the supposed fourth satellite was last night. $10^h\ 47'$, I can see no fourth satellite near the second where it would be now if it had been a real satellite. With the assistance of a field bar to hide the planet; and a power of 300, I can see the first and second satellites very steadily, even the very first moment I look into the telescope.

‡ The first satellite was $76\frac{2}{3}$° sp; the second was $77\frac{1}{4}$° np. It is very strange that the third supposed satellite should not have been attended to when two observations are given to prove that the supposed fourth was not a satellite.

1790, January 20, $12^h\ 5'$. The first and second satellites are in the places I had calculated; a third satellite is 45° np, and in a line with the planet and the second satellite.

‡ The first satellite was $77\frac{1}{4}$° sf; the second was $54\frac{1}{2}$° np. The third was not accounted for.

1790, February 6, $9^h\ 28'$. I viewed the planet and satellites with three concave eye glasses, power about 240, 320, and 460. I see very clearly with these glasses. Cloudy.

‡ The first was $89\frac{3}{4}$° sf; the second was 64° sp.

1790, February 9, $9^h\ 19'$. By a configuration the first satellite is at a considerable angle nf; the second at a great angle sf. A third is in a line with the planet and the second satellite; its distance from the planet by the configuration is about twice that of the second satellite.

‡ The first was $48\frac{3}{4}$° nf; the second was $61\frac{1}{2}$° sf; the third was $61\frac{1}{2}$° sf; two succeeding observations are decisive that

the supposed third satellite was not a star remaining in its place.

1790, February 11, 8° 30′. The satellites are in the places I had calculated. 8^h 56′, the small star of the 9th of February I believe is wanting; at least I cannot see it though the weather is very clear, but windy. An additional third and fourth are pointed out.

‡ The first satellite was 74° np; the second was 7° nf. The third and fourth of this night were found to be stars.

1790, February 12, 11^h 27′. The first and second satellites are in the places I had calculated. The third and fourth of last night are small fixed stars remaining in their places. The supposed third satellite of the ninth is not in the place where I saw it that night.

‡ The first satellite was 27° np; the second was $48\frac{1}{3}$° nf.

1790, February 16, 8^h 2′. The first and second satellites are in the places I had calculated; the situation of a supposed third is described.

‡ The second was $56\frac{1}{3}$° np; the supposed third proved to be a star.

1790, February 17. A configuration of stars situated in the planet's path is delineated.

1790, March 3, 7^h 58′. The first satellite is 40° sp. 8^h 42′, the second is 3 or 4° np.

‡ The first satellite was 56° sp; the second was $0\frac{1}{3}$° np.

1790, March 5, 10^h 38′. The first and second satellites are in the places I had calculated; a 3d, with 600 is 56° sf; a fourth is delineated.

‡ The first satellite was 63° sf; the second was 66° sp; the third and fourth proved to be stars.

1790, March 8, 10^h 43′. Forty feet telescope. I saw the satellites with great ease. The speculum being extremely tarnished, I did not expect to have seen so well as I did.* Twenty feet telescope. The first satellite is 85° 7′ nf. 8^h 39′, the second is 67° 36′ sf. My wire is too fine and the power 460 too high for great accuracy.

‡ The first satellite was $79\frac{1}{3}$° nf; the second was $59\frac{1}{2}$° sf.

1790, April 3, 9^h 39′. The first satellite is on the opposite side of the second; the position of the second is 77° 53′ sf.

‡ The first satellite was 75° nf; the second was $76\frac{2}{3}$° sf.

1791, January 31, 11^h 5′. The second satellite is 74 or 75° np. A supposed satellite in opposition to the second, and at double its distance from the planet, is marked in the configuration.

‡ The first satellite was $0\frac{3}{4}$° nf; its distance was 336, and not being noticed it was probably invisible; the second was $78\frac{3}{4}$° np; the supposed exterior satellite was $78\frac{3}{4}$ np.

1791, February 2, 8^h 23′. The first satellite is about 70° nf. 8^h 10′, the second is gone with the planet from the stars of the configuration of the 31st of January.

‡ The first satellite was 81° nf; the second was 30° np. The lettered stars of the configuration were all named as being left in their places, but the supposed exterior satellite of that day is not mentioned among them.

1791. February 4, 8^h 13′. The second satellite is 40° 48′ sp, but the measure is imperfect and may be out 5 or 6 degrees; a supposed satellite was marked, and a small star pointed out in the path of the planet.

* See Phil. Trans. for 1814, p. 275. A note relating to the polish of the 40 feet mirror.

‡ The second satellite was 52° sp. The supposed satellite was found to be a star.

1791, February 5, 11^h 5′. The first satellite is 20° sp. 10^h 45′, the second is 65° sp. With 600, third, fourth and fifth satellites are marked: but as they are also visible with 300, they are probably stars.

‡ The first satellite was 41° sp; the second was $74\frac{1}{2}$° sp. The third, fourth and fifth, were lost for want of subsequent observations.

1791, February 22, 8^h 23′. I cannot perceive the first satellite, probably owing to its nearness to the planet; I am pretty sure the orbits are contracted, so that the planet is approaching towards their node. 7^h 30′, a measure of the position of the second satellite is 36° 18′ sf.

‡ The first satellite was $8\frac{3}{4}$° sp; distance 333, which may account for its not having been seen. The second was 39° sf.

1791, February 23, 7^h 59′. Position of the first satellite 56° 33′ sp.

‡ The first was $60\frac{1}{2}$° sp; the second was $7\frac{1}{2}$° nf; the distance was 331, at which the satellite is sometimes invisible.

1791, March 1, 11^h 47′. The two satellites are in the places I had calculated.

‡ The first was $73\frac{1}{3}$° np; the second was $20\frac{3}{4}$° np.

1791, March 2, 9^h 18′. The first satellite is hardly to be seen; it seems to be in about the most contracted part of its orbit; the second is only about two diameters of the planet from the edge of the disk, but the estimation cannot be very accurate, as I am obliged to hide the planet to see the satellite.

‡ The first was $37\frac{1}{3}$° np; its distance 377. The second was $22\frac{1}{2}$° sp; its distance 358.

1791, March 5, 7^h 56′. The first satellite is about 75° sp; the second about 85° sp; third, fourth, and fifth satellites were pointed out.

‡ The first satellite was 87° sf; the second was $89\frac{1}{2}$° sf; the third, fourth and fifth proved to be large stars, the nearness of the planet having diminished their lustre when observed as satellites.

1791, March 6, 12^h 2′. The first satellite is much nearer the planet than it was last night; the second is also nearer, but not much.

‡ The first satellite was $50\frac{1}{2}$° sf; the second was $70\frac{1}{2}$ sf.

1791, March 9, 9^h 52′. The first satellite, with a wire for the parallel and 300, is about 86° nf. The position being so near the perpendicular cannot be much out. By the micrometer it is 86° 25′ nf. 9^h 36′, the second is on the following side; it is nearer the planet than the first, and on that account appears smaller.

‡ The first satellite was $86\frac{3}{4}$° nf; distance 599; the second was $32\frac{1}{2}$° nf; distance 379.

1791, April 4, 8^h 43′. The first satellite is 84° 56′ nf; the second was not observed.

‡ The first was $82\frac{3}{4}$° nf; the second was $9\frac{3}{4}$° sf; its distance being 343 it might not be visible.

1791, December 19, 11^h 45′. I do not perceive the first satellite; the second is about 75° nf.

‡ The first was $7\frac{1}{3}$° sf, and its distance being 263, it was therefore invisible. The second was $82\frac{1}{2}$° nf.

1792, January 27, 11^h 58′. The first satellite was not observed; the second is about 40° nf, the estimation may be out 6 or 8 degrees. Cloudy.

‡ The first satellite was $20\frac{1}{3}°$ np; its distance being 283 it could not be seen. The second was $62\frac{1}{4}°$ nf.

1792, February 12, 8^h 28′. The first and second satellites are in the same line, and I measured their position together, it is 88° 19′ np; a supposed third is 84° 23′ sp.

‡ The first satellite was $86\frac{1}{3}°$ np, and its distance 576; the second satellite was $83\frac{3}{4}°$ np; the third proved to be a star.

1792, February 13, 8^h 42′. Forty feet reflector, with 360, I saw the disk of the planet very well defined. Twenty feet. The satellites are advanced in their orbits; the first is drawn much nearer to the planet than it was yesterday; a very small star is 41° 22′ nf.

‡ The first satellite was $56\frac{1}{2}°$ np; its distance 389; the second was $66\frac{1}{3}°$ np. The very small star was left in its place.

1792, February 20, 12^h 57′. The first satellite is 89° 58′ nf; 13^h 8′, the second is 53° 21′ sf; a supposed third is 66° 17′ np.

‡ The first satellite was $83\frac{1}{3}°$ nf; the second was $53\frac{1}{3}°$ sf. By an increase of 25 or 30° in the angle of the third it was the same evening proved to be a star.

1792, February 21, 9^h 10′. Position of the first satellite 73° 52′ np.—9^h 30′, I suspected the second satellite to be in its calculated place, but even 600 would not verify it.

‡ The first satellite was $78\frac{2}{3}°$ np; the second was $17\frac{1}{3}°$ sf; distance 292.

1792, February 26, 11^h 30′. The position of the first satellite is 42° 49′ sf.—8^h 2′, that of the second is 73° 49′ np. A very small star between the planet and the second satellite is pointed out, and another towards the south at double the distance of the first is marked in the configuration.

‡ The first satellite was $52\frac{3}{4}°$ sf; the second was $75\frac{1}{3}°$ np. The small star was left in its place; but the distant one is not accounted for.

1792, February 28, $10^h\ 52'$. The position of the first satellite is $69°\ 43'$ nf. The second was not seen.

‡ The first satellite was $65\frac{1}{2}°$ nf; the second was $3\frac{1}{2}°$ sp; distance 293, and therefore invisible.

1792, March 15, $10^h\ 3'$. I cannot see the first satellite with 300, 480, nor with 600. The second satellite is $73°\ 22'$ sp.

‡ The first satellite was $17°$ sf; its distance was 302 and therefore invisible; the second was $74\frac{1}{2}$ sp.

1792, March 18, $8^h\ 19'$. The first satellite is $82°\ 35'$ np. $8^h\ 37'$, the second is $60°\ 16'$ sf.

‡ The first satellite was $81\frac{1}{4}°$ np; the second was $56°$ sf.

1792, March 19, $8^h\ 20'$. The first satellite is $38°\ 4'$ np, I see it very well notwithstanding it is near the planet. $8^h\ 42'$, I cannot see the second with 300. With 480 I see it very well; I see it also with 800 and 1200; I tried 2400 and 4800, but a whitish haziness in the air prevents my seeing it with these powers.

‡ The first was $46°$ np and its distance 364; the second was $15°$ sf; its distance 299 which accounts for the difficulty of seeing it.

1792, March 23, $8^h\ 21'$. I see the first satellite through flying clouds; the second is $89°\ 21'$ np.

‡ The first satellite was $61\frac{2}{3}°$ sf; distance 430. The second was $88\frac{2}{3}°$ np.

1792, March 27, $11^h\ 6'$. The first satellite (miscalled a very small star) is about $80°$ np; the second s by the configuration about $45°$ sp; a third (miscalled the first) was pointed out.

‡ The first satellite was 70° np; the second was $46\frac{1}{2}$° sp; the third was not accounted for.

1792, March 30, 11^h 18′. The first satellite by the configuration is at a great angle sp; the second is at a great angle sf.

‡ The first satellite was $78\frac{1}{2}$° sp; the second was 83° sf. The first satellite (miscalled a star the 27th) is gone from the place where it was.

1793, February 5, 9° 18′. Neither the first nor second satellites are visible. A very small star is 19° 3′ sp.

‡ The first satellite was 30° sp; distance 282; the second was 23° sf; distance 2 45. There is no subsequent observation of the small star.

1793, February 7, 9^h 38′. The first satellite is 79° 39′ sp. 9^h 20′, the second is 59° 51′ nf. The wind being very troublesome, the measures cannot be very accurate: The difficulty was in finding the parallel.* I viewed the planet with 240, 320, 480, 600, 800 and 1200, but saw no satellites nearer than the two known ones. The north following satellite being farther from the planet than the south following one, I take it to be the second; the difference of their distance appeared plainest with 1200. I viewed the planet also with 2400, 3600, 7200.

‡ The first satellite was $86\frac{3}{4}$° sp. Distance 590. The second was $68\frac{1}{4}$° nf; distance 513; and supposing the radius of the orbit of the first to be to that of the second as 3 to 4, we have the apparent distance of the first to that of the second as 177 to 205.

* Telescopic vision in windy weather is generally very perfect, and except in cases which require an uninterrupted steadiness of the instrument, will admit of the highest magnifying powers.

1793, March 8, $11^h\ 21'$. The first satellite is about 65° nf; the second is 90° nf; a third is about 75° nf.

‡ The first was $57\frac{3}{4}$° nf; the second was $89\frac{2}{3}$° nf; the third was a star.

1793, March 9, $10^h\ 35'$. The first satellite is 85° nf; the second is about 82° np; a third is about 65° sp.

‡ The first was 80° nf; the second $77\frac{1}{2}$° np; there is no subsequent observation of the third.

1793, March 14, $9^h\ 37'$. The first and second satellites are seen in their places; the situation of a third and fourth is pointed out. The first satellite is brighter than the second.

‡ The first was $89\frac{2}{3}$° sf, and 16° 26′ from its greatest elongation; distance 574. The second was 80° sp, and 7° 56′ from its greatest elongation; distance 588. The superior brightness of the first therefore could not arise from its greater distance. The third and fourth supposed satellites had no subsequent observations.

1793, April 3, $10^h\ 53'$. The first satellite is 50° nf; the second is 80° nf.

‡ The first was $52\frac{2}{3}$° nf; the second was 79° nf.

1794, February 21, $8^h\ 24'$. The first satellite is about 88° nf; the second is about 86° nf.

‡ The first was 89° nf; the second 85° nf.

1794, February 25, $8^h\ 24'$. By a configuration the first satellite is at a great angle sp; and its distance from the planet is greater than that of the second, which is at a much smaller angle sp. Several small stars are pointed out.

‡ The first satellite was 84° sp; distance 593. The second was $47\frac{3}{4}$° sp; distance 323. The stars were left in their places.

February 26, 8^h 28′. The first satellite is 70° 53′ sf. 8^h 7′, the second is 66° 56′ sp; many small stars are pointed out.

‡ The first satellite was 78° sf; the second was $67\frac{1}{3}$° sp; the stars remained in their places.

1794, February 28, 9^h 43′. The first satellite is 62° 55′ nf. 9^h 26′, the second is 86° 44′ sp. 8^h 15′, there is a very small star which I did not see the 26th; it is brighter than a lettered star not far from it. Its position is pointed out by the stars of the configuration.

‡ The first satellite was $61\frac{1}{2}$ nf; the second was $87\frac{1}{2}$° sp. The stars remained in their places. The position of the small star of the 26th, by identification was about 24° nf.

1794, March 2, 8° 25′. The first satellite seems to be at its greatest elongation; the second satellite was not seen.

‡ The first was 86° np; distance 507; the second was $55\frac{3}{4}$° sf; distance 275, therefore not visible.

1794, March 4, 11^h 22′. I can see neither the first nor the second satellite. A third satellite is 61° 32′ nf. Many small stars are pointed out.

‡ The second satellite was $57\frac{1}{2}$° nf; its distance 383; it was therefore visible, and its position agrees with the measure taken of a satellite miscalled the third: the inaccuracy of my tables in 1794, occasioned the mistake. The small stars remained.

1794, March 5, 11^h 10′. The first satellite is 75° 50′ sp. 10^h 57′, the second is 72° 27′ np. There is no star in the place where the supposed third was last night. Many small stars are again pointed out.

‡ The first was 76° sp; the second was $72\frac{1}{4}$° nf. The absence of the miscalled third confirms the mistake, and

is a proof of the great attention that was paid to ascertain the nature of supposed satellites. The small stars remained.

1794, March 7, 11^h 18′. I cannot perceive the first satellite. 10^h 57′, the second is nearer the planet than it was the last time I saw it. Small stars are pointed out.

‡ The first was 63° sf; distance 320, invisible. The second was $87\frac{1}{3}$° np. There is no subsequent observation of the small stars.

1794. March 17, 7^h 38′. I can see neither of the two satellites.

‡ The first was 41° nf; distance 292, invisible. The second was $38\frac{1}{3}$° nf; distance 285, invisible.

1794, March 21, 11^h 53′. I cannot see the first satellite. I looked at several different hours for it. 10^h 53′, the second satellite is 88° 8′ np. 9^h 19′, the place of a small suspected star is pointed out, but it cannot be verified with 460 and 600.

‡ The first satellite was 27° sp; distance 243, invisible. The second was $81\frac{1}{2}$° np. The suspected star was seen in its place the following night.

1794, March 22, 8^h 47′. There is no mention of the first satellite; the second is 61° 46′ np.

‡ The first was $66\frac{1}{4}$° sp, and its distance being 480, it must have been seen and taken for a star. The second satellite was $60\frac{3}{4}$° np; distance 311.

1794, March 23, 8^h 32′. The first satellite is one of two small stars that are south of the planet; it is the preceding and largest of the the two. 8^h 42′, the second satellite is not visible.

‡ The first was 82° sp, 1° 26′ past its greatest elongation The second was 1° np, 1° 49′ past its shortest elongation, distance 207; invisible.

1794, March 26, 9^h 2′. Position of the first satellite 61° 53′ nf, as accurate as the faintness of the satellite will permit. 8^h 48′, the second satellite is 77° 0′ sp. very accurate. 9^h 17′, I suspect a third satellite directly north a little farther from the planet than the first, and the power 480 almost verifies the suspicion. 9^h 26′, with 600 I still suspect the same, but cannot satisfy myself of the reality. 11^h 24′, I see the supposed third satellite perfectly well now; it is much smaller than the first, and in a line with the planet and the first. An extensive configuration is delineated.

‡ The first satellite was $56\frac{2}{3}$° nf; the second was $79\frac{1}{2}$° sp; the third satellite being in the position of the first at 11^h 14′, must have been $59\frac{1}{2}$° nf.

1794, March 27, 10^h 25′. The first satellite is 75° 59′ nf. 10^h 12′, the second is 88° 35′ sf. 8^h 15′, the small star observed last night at 11^h 14′ is gone from the place where I saw it. From its light last night compared to a star marked *r* in the configuration which to night is very near the planet, and scarcely visible, I am certain that it must be bright enough to be perceived immediately, if it were in the place pointed out by the configuration. 11^h 19′, I have many glimpses of small stars, one of them is in a place a little north following the first satellite, agreeing with what would probably be the situation of the third satellite of last night if it had moved with the planet. A supposed third of this evening is preceding the first satellite, but nearer the planet. A supposed fourth is sf; its distance is almost double that of

the second satellite. Some other small stars or supposed satellites are seen to the south at a good distance.

‡ The first satellite was 79° nf; the second was $81\frac{1}{4}$° sf. The fourth proved to be a star.

1794, March 28, 9^h 12′. The first satellite is 88° 31′ np. 9^h 1′, the second is 82° 7′ sf.

‡ The first satellite was 87° np; the second was 78° sf.

1794, April 1, 9^h 14′. The first satellite i 83° 2′ sp. 9^h 23′, the second is 70° 26′ nf.

‡ The first satellite was $87\frac{1}{4}$° sp; the second was 72° nf.

1796, March 4, 11^h 10′. The Georgian planet is about 13′ of space preceding and 5 or 6′ north of a nebula. An extensive configuration was made, but no satellites were noticed.

‡ The nebula was No. 272 of the first class of my third catalogue. (See Phil. Trans. for 1802, page 505.) The first satellite was $42\frac{1}{3}$° sp; distance 158, invisible. The second was $87\frac{1}{2}$° np, distance 364, and after an interval of near two years might possibly be overlooked. The stars pointed out remained in their places.

1796, March 5, 12^h 25′. The first satellite is 72° 20′ sp. 10^h 53′, I suspect a very small star between two of the lettered stars of last night's configuration which at the time it was made was not there. I had a pretty certain glimpse of it.

‡ The first satellite was 75° sp.—10^h 25′, the second was 72° np; distance 123, invisible. By the configuration the suspected star was at a considerable distance, about 72° np.

1796, March 9, 11^h 35′. The first satellite is 70° 36′ nf. 11^h 22′, the second is 83° 35′ sp. As the probability is that

other supposed satellites move in the same plane with the first and second, I chiefly look for them in the direction of the position of their orbits which is now nearly a straight line; a star that may possibly be a distant satellite is pointed out.

‡ The first satellite was $70\frac{2}{3}°$ nf. The second was 81° sp; the star remained in its place.

1796, March 10, 11^h 43′. The first and second satellites by a configuration, are not far from being in opposition, the first not being come to a line drawn from the second through the planet. With 600 there is no star between the satellites and the planet that may be supposed to be an inner satellite; with this power the satellites are very large and visible, I see them better than with a lower.

‡ The first satellite was $79\frac{3}{4}°$ nf; the second was $86\frac{1}{2}°$ sp.

1796, March 27, 10^h 6′. The first satellite is in the place I had calculated.

‡ It was $75\frac{1}{2}°$ nf.

1796, March 28, 10^h 7′. The first and second satellites are in the places I had calculated; the apparent contraction of their orbits is such as to approach to a straight line.

‡ The first satellite was 84° nf; the second was 74° nf.

1796, April 4, 11^h 16′. The first satellite is not visible; the second is near a small sp. star. There is no star in the transverse of the apparent elliptical orbit that could be taken for a satellite, unless that near the second should be one going towards its greatest elongation, or coming from it.

‡ The first satellite was 68° nf; its distance 415; how it happened not to be visible I cannot account for; the configuration has no star near the place; the second satellite was $76\frac{1}{3}°$ sp. The star near the second remained in it former situation.

1796, April 5, 10^h 48′. The first and second satellites are apparently in opposition, the same wire covers them both and the planet. There is no star in the line of the transverse that can be taken for a satellite: the night being beautiful, I examined that line with 300 at a distance, and with 600 within the orbit of the two satellites.

‡ The first satellite was $74\frac{2}{3}°$ nf; the second was $81\frac{2}{3}°$ sp.

1797, March 15, 9^h 44′. The first and second satellites are not far from their opposition; by the configuration they are short of it.

‡ The first was $78\frac{3}{4}°$ sp; the second was $81\frac{1}{4}°$ nf.

1797, March 17, 9^h 51′. The first and second satellites are both invisible; the night is very beautiful and I have a field bar to hide the planet; but notwithstanding this, I cannot see either of the satellites; many stars are pointed out.

‡ The first was $69\frac{3}{4}°$ sf; distance 110, invisible. The second was 76° np; distance 126, invisible. The stars had no subsequent observation.

1797, March 21, 10^h 9′. The first satellite is not visible; the second is nearly at its greatest elongation, it is about 70° south preceding; many stars are pointed out.

‡ The first satellite was $88\frac{1}{2}°$ np; distance 240, invisible; the second was $79\frac{1}{2}°$ sp; distance 588. The stars remained.

1797, March 23, 10^h 28′. With 320, I see neither of the satellites. 10^h 32′, having just been told where the second should be, I perceived it in its place; with 600 I see it very well; many stars are pointed out.

‡ The first satellite was 76° sp; distance 518; it does not appear why it could not be seen. The second was 88° sp; distance 298. The stars remained.

1797, March 25, 10^h 40′. With 320 I cannot perceive either of the satellites; with 600 I can see neither of them; many stars are pointed out.

‡ The first satellite was 87° sp; distance 320, invisible. The second was $66\frac{2}{3}$° nf; distance 251, invisible. The stars remained.

1797, March 28, 10^h 36′. The two satellites seem to be nearly in a line drawn from the second to the centre of the planet; the second is 80° 26′ nf. There is an exceeding small star about four times the distance of the second satellite in the line of the greatest elongation, I do not remember to have seen it among the lettered stars which are pointed out, the 25th.

‡ The first satellite was $78\frac{1}{2}$° nf; distance 597. The second was $80\frac{1}{3}$° nf; distance 592. There is no subsequent observation of the small star.

1797, March 29, 9^h 39′. I see one of the satellites. Cloudy. I suppose it to have been the second.

‡ The first was 83° nf; the second was $83\frac{1}{3}$° nf.

1798, February 6, 11^h 29′. The second satellite (miscalled a star) is at a great angle sp; three stars are pointed out.

‡ The first satellite was $74\frac{1}{3}$° nf; distance 251; invisible; The second was 79° sp; distance 552. The three stars remained.*

1798, February 7, 11^h 7′. I cannot see the first satellite. The second is at a great angle sp near one of the stars marked down last night, which is now so small that I cannot distin-

* The planet being past the node, the angular distance of the satellites from zero, and their apparent motions are inverted.

guish it from the satellite. An extremely small star preceding a line that joins two lettered stars may be an exterior satellite. Position of the satellite (called the 6th) $64° 41'$ np. $12^h 36'$, there is no star in the path of the planet which to morrow or next day can be taken for this 6th satellite.

‡ The first satellite was $85°$ sp; distance 159, invisible. The second $78\frac{1}{3}°$ sp; distance 592. The supposed satellite, called the 6th by way of readily referring to it, and also partly to express its distance, was found to remain in its observed place.

1798, February 11, $9^h 17'$. The path of the planet is marked by a configuration of lettered stars, taking in those from which it comes and those towards which it will go.

$11^h 35'$, the situation of a supposed exterior satellite (called the 5th) with regard to the lettered stars is pointed out. It is excessively faint, but the night is very beautiful.

$11^h 46'$, the position of the 5th satellite is $89° 19,5$ nf; but the satellite is so faint that the measure cannot be very accurate.

$12^h 16'$, I cannot see either of the two old satellites. There is an extremely small north preceding star x, which may be a more distant satellite; it is much smaller than the 5th, and will therefore become invisible when the planet comes near it.

‡ The first satellite was $89\frac{3}{4}°$ nf; its distance was 41, and it was therefore invisible. The second was $79\frac{1}{2}°$ nf; its distance was 241; and it was therefore also invisible.

1798, February 13, $10^h 17'$. The old satellites are in the place I had calculated. The 5th satellite and the small star x, observed February 11, are not visible; but the weather is very indifferent.

11^h 49′, I do not see the 5th satellite where it was February 11.

12^h 0′, the position of the two old satellites is 76° 48′ nf.

12^h 15′, I see the extremely small star x remaining in its former place.

12^h 44′, the first and second satellites are exactly in a line pointing to the centre of the planet. A second measure of their position is 75° 45′. I cannot see the 5th satellite in the place where it was February 11.

‡ The first satellite was $78\frac{1}{4}$ nf; distance 594. The second was $78\frac{1}{2}$° nf; distance 576.

1798, February 15, 11^h 21′. There is a very small star in the line of the greatest northern elongation, it may possibly be an interior satellite, the first and second being invisible.

I see the extremely small star x of the 11th perfectly well, but the 5th satellite of the same night is gone from the place where it was that evening. It was considerably brighter than x, so that if it were in its place, I must certainly see it.

11^h 41′, the star which at 11^h 21′ I supposed might be an interior satellite is too far from the planet; it may possibly be the 5th satellite of the 11th on its return from the northern elongation towards the planet.

I believe there is another satellite or star between this last mentioned one and the planet; I do not suppose the second satellite to be visible, otherwise it would agree well enough with the situation of the star between the 5th and the planet. By the configuration the intermediate star is at about half the distance of the farthest of the two.

12^h 13′, position of the supposed 5th satellite 84° 49′ nf.

‡ The first satellite was 73° nf; distance 124, it was there-

fore invisible and could not be the supposed 5th satellite. The second was 77° nf; distance 455; it was therefore visible, and agrees very well with the satellite miscalled the 5th; the star between the second satellite and the planet must have been an interior satellite at its greatest northern elongation. At the time of observation, my defective tables made me suppose the nearest of the two to be the second satellite.

1798, February 16, 9^h 25′. The supposed fifth satellite observed last night at 11^h 41′, I believe is gone from the place where I saw it at that time. The night is very beautiful; the planet however is still low, and I shall look for it again when it is higher.

10^h 57′, the supposed fifth satellite is gone from its former place. It was so visible last night when near the planet, that I should certainly see it without difficulty if it remained in the same place, as the planet is now removed from it.

11^h 5′, the first and second satellites are invisible.

11^h 12′, there is a very faint satellite in the southern elongation; probably the sixth; and if it be the sixth satellite it is probably a little before or after its greatest elongation. It is excessively faint.

12^h 27′, the weather is not so clear now, though still fine, but the sixth satellite cannot be seen; it is plain, therefore, that the least haziness will render it invisible.

‡ The first satellite was $80\frac{1}{3}$° sp; distance 289, invisible; the second was $75\frac{1}{2}$° nf; distance 244, invisible. The interior satellite observed the fifteenth, being taken for the second, was not looked after; but as the supposed fifth was scrupulously ascertained to be removed, the interior satellite, had it been a star remaining in its former situation, must unavoidably

have been seen; for by the configuration they could not be much more than a diameter of the planet asunder.

1798, February 18, 9^h 19′. I see the sixth satellite observed February 16, at 11^h 12′, it has left the place where it was at that time. It is nearer the planet than it was that evening, I suppose it therefore to be on its return from its southern elongation.

There is a seventh satellite near the sixth, rather a little fainter than the sixth; a supposed eighth satellite is pointed out.

11^h 25′, the position of the sixth satellite is 80° 53′ sp.

11^h 31′, with 480 I see the satellite near the sixth perfectly well; the distance between the two is about $\frac{3}{4}$ or one diameter of the planet.

11^h 44′, I see the satellite much better with 600; that which is farthest from the planet is the largest.

‡ The first satellite was 78° sp; distance 563. This therefore might be the satellite which was seen near the sixth. The second satellite was $80\frac{1}{2}$ sp; distance 290, and was invisible. The supposed eighth, proved to be a star.

1798, February 19, 11^h 12′. The first satellite is invisible; the second is near the greatest elongation.

‡ The first was 81° sp; distance 269; invisible. The second was $79\frac{1}{2}$° sp; distance 490.

1798, February 22, 9^h 9′. The first satellite by its distance is not far from its greatest northern elongation; it is very large. There is a satellite to the south exactly opposite to the first; it is very small but may be the second. The moon is too bright to see very faint satellites.

‡ The first satellite was 78° nf; distance 594. The second

was 78° sp; distance 431. The small appearance of the second satellite is not easily to be accounted for; its distance from the planet was not much less than that of the first; for if the greatest elongations of the satellites be as 3 to 4, the above distance will be 1782 to 1724.

1798, February 26, 9^h 52′. The first and second satellites are in opposition; the first being sp, the second nf. The moon is so bright that their light is very feeble. Position 79° 53′ from sp to nf. The first satellite is small, the second is large.

‡ The first satellite was 78½° sp; distance 584. The second was 79° nf; distance 527. The different proportional light of the satellites in different situations, will lead us to suppose that they have a rotation on their axes. The twenty-second, when the second satellite was 78° sp, it was fainter than the first, and this evening when it was nf, it was brighter.

1798, March 11, 8^h 13′. With 300 the first and second satellites are close together, like a very faint double star of the first class. The second satellite is the most north of the two.

9^h 45, the position of the two satellites is 78° 15′,3 nf. There is hardly a division between them.

‡ The first satellite was 78¼° nf; distance 580. The second was 78¼° nf; distance 445; and supposing the diameter of the orbits of the two satellites to be as 3 to 4, their distances at the time of observation would be as 174 to 178.*

1798, March 12, 9^h 11′. The first satellite is nearly at the same distance from the planet as it was last night; the second

* The angular distance of the satellites from zero and their apparent motions are reverted.

is farther from the planet. The two satellites and the centre of the planet are exactly in a line. Their position is 78° 12′,6. With 480 I had a glimpse of a south preceding satellite; but could not verify it with 600.

12^h 6′. Distance of the second satellite 50″,02. I contrived to throw a little light upon the wires, as the satellite was bright enough this evening to bear it.

‡ The first satellite was $78\frac{1}{4}$° nf; distance 528. The second was $78\frac{1}{4}$° nf; distance 577.

1798, March 13, 11° 46′. The first satellite is invisible. The second is much nearer the planet than it was last night. The weather is not clear, owing to easterly winds.

‡ The first satellite was $78\frac{1}{4}$° nf; distance 170; invisible. The second was $78\frac{1}{4}$° nf; distance 577.

1798, March 14, 11^h 55′. The first satellite is invisible; 8^h 31′, the second is still at a considerable distance south preceding.

11^h 47′, twenty-five reflector, power 200. The Georgian planet is better defined in this instrument than I have ever seen it before. With 300, its disk is as sharp and well defined as that of Jupiter. The second satellite is brought to a sharp point. A little while ago I had a glimpse of a south preceding satellite, and just now I have seen it again. 12^h 0′, I cannot verify the satellite, but can hardly believe it a deception.

Twenty feet reflector, power 300. I tried to measure the distance of the second satellite, but its present faintness will not afford light enough to see the wires of the micrometer.

‡ The first satellite was $78\frac{1}{4}$° sp; distance 260, invisible; but the 25 feet telescope with a mirror of 24 inches in

diameter, it appears, had light enough to show it. The second satellite was $78\frac{1}{4}°$ nf; distance 473.

1798, March 16, 8^h 37′. I see a south preceding satellite at a good distance; it may be the first at its greatest elongation, but it is certainly smaller than it should be; unless the state of the air should be worse for seeing than it appears to be.

9^h 31′, the distance of the first satellite is 36″,05. The satellite is so faint that it is impossible to be very accurate; it will not bear any light to the wires.

11^h 34′, twenty-five feet reflector. With 300 I see the satellite very distinctly, but the evening is not fine.

‡ The first satellite was $78\frac{1}{4}°$ sp; distance 576. The second at 8^h 28′ was $78\frac{1}{4}°$ sp; distance 13; invisible.

1798, March 18, 8^h 26′. The first satellite is invisible; the second is south preceding at a considerable distance; it is farther off than the greatest elongation of the first.

‡ The first satellite was $78\frac{1}{4}°$ nf; distance 51; invisible. The second was $78\frac{1}{4}°$ sp; distance 484.

1798, March 19, 9^h 51′. I see a north following satellite which I suppose to be the first. The second is near its south preceding greatest elongation. Distance 49″,90, I can only apply a very distant lantern, which will hardly give light enough to show the wires. The satellite is not so bright in its southern elongation as it was March 12th in its northern one, though the weather is now very beautiful. In the south preceding elongation is a distant star that may be a satellite; many other stars are pointed out.

‡ The first satellite was $78\frac{1}{4}°$ nf; distance 445. The

second was $78\frac{1}{4}^\circ$ sp; distance 588. The stars remained in their places.

1798, March 21, $10^h\ 25'$. The first satellite is north following; it is faint, and at nearly the same distance from the planet as it was in March 19. The second satellite is near one of the stars pointed out the 19th, both being at the same distance from the planet. $10^h\ 40'$, there is such a multitude of small stars in the neighbourhood of the planet, that it would be endless to look for the additional satellites among them.

‡ The first satellite was $78\frac{1}{4}^\circ$ nf; distance 438. The second was $78\frac{1}{4}^\circ$ sp; distance 409.

1798, March 22, $10^h\ 35'$. The first and second satellites are both invisible.

‡ The first was $78\frac{1}{4}^\circ$ nf; distance 58; invisible. The second was $78\frac{1}{4}^\circ$ sp; distance 173; invisible.

1798, April 6, $8^h\ 31'$. The first satellite is north following; I suspect the second to be between the first and the planet, but cannot verify the suspicion. There is a supposed south preceding satellite, but it is too near the planet to be seen steadily.

‡ The first satellite was 78° nf; distance 564. The second was $75\frac{1}{3}^\circ$ nf: distance 230; it was therefore the satellite suspected between the first and the planet. The supposed south preceding satellite was lost among the numerous small stars.

1798, April 7, $9^h\ 26'$. There are two satellites north following; they are very near together. The distance between them is less than half the diameter of the planet. The centre of the planet and the two satellites are exactly in a line.

There are so many small stars that it is next to impossible to look for the additional satellites.

‡ The first satellite was 79° nf; distance 546. The second at $8^h\ 45'$ was $77\frac{1}{3}$° nf; distance 453.

1798, April 8, $10^h\ 19'$. There is no satellite visible between the second and the planet; the second satellite is north following, at a greater distance from the planet than last night. There is a very small star at a little more than twice the distance of the second satellite north following.

‡ The first satellite was $80\frac{1}{2}$° nf; distance 239; invisible. The second was 78° nf; distance 576. The small star remained in its place.

1798, April 9, $9^h\ 34'$. I cannot see the first satellite. The second is at a distance north following, rather farther from the planet than last night. With 480 and 600, there is no satellite between the second and the planet.

‡ The first was $74\frac{1}{2}$° sp; distance 171; invisible. The second was 79° nf; distance 578.

1798, April 11, $9^h\ 8'$. The first satellite is south preceding at a considerable distance; but not at its greatest elongation. I cannot see the second satellite. I suspect a very small star in the line of the north following greatest elongation, a little farther from the planet than the first satellite. With 480 I cannot verify the suspicion.

‡ The first satellite was $78\frac{1}{2}$° sp; distance 587. The second was $80\frac{1}{4}$° nf; distance 247; invisible. This satellite could hardly be the suspected star, as it was but at little more than half the distance of the first satellite from the planet.

1798, April 12, $9^h\ 54'$. I cannot see the first satellite, nor the second. With 480 there is no satellite either new or old

visible. The night seems to be very clear, but the wind is in the north-east.

‡ The first satellite was $80\frac{2}{3}$° sp; its distance was 382, and it ought to have been seen. The second was 61° sp; distance 46; invisible; if the north-east wind, which is always unfavourable for astronomical observations, prevented my seeing the first satellite, it could not be expected that the suspected star of the eleventh would be visible.

1798, April 13, 9^h 5′. The first satellite was not seen. There is a south preceding satellite at a little greater distance than half the greatest elongation of the first. It took some time to verify its existence. 400 shows it very well; it is the second satellite. The very small stars in this neighbourhood are so numerous, that it is impossible to look for the new satellites among them. A great many of the stars are pointed out.

‡ The first was 1° sf; distance 16; invisible. The distance being so small, the method here used is not sufficient to give an accurate position. The second satellite was 76° sp; distance 301.

1798, May 3, 10^h 3′. The first satellite is north following. 10^h 7′, I suspect another north following a little nearer the planet than the first; 460 almost verifies it.

‡ The first satellite was $79\frac{1}{3}$° nf; distance 556. The second, which was the suspected one, was $74\frac{1}{2}$° nf; distance 259; it was therefore seen at a distance considerably less than half the greatest elongation.

1799, April 3, 9^h 51′. I viewed the Georgian planet with 157 and 300; one of the satellites is about 10° sp.

‡ The first satellite was 77° sp; distance 581; the second

was 66° sp; distance 172; this being invisible and the first at so great an angle, a star must have been taken for a satellite, which might well happen after an interval of eleven months. The observation was chiefly made to try two newly polished mirrors.*

1799, April 8, 9^h 51′. Both the old satellites are in a line near the greatest north following elongation. The nearest is very faint.

‡ The first satellite was $74\frac{1}{3}$° nf; distance 452; it was the faint satellite and the nearest of the two. The second was $75\frac{3}{4}$° nf; distance 529.

1800, March 26, 11^h 0′. With a new mirror and power 300 I saw the planet beautifully well defined, and one of its satellites south preceding.

‡ The first was $74\frac{3}{4}$° sp; distance 566, it was therefore the observed satellite. The second was $31\frac{2}{3}$° nf; distance 134; invisible.

1800, April 26, 9^h 30′. I see two satellites almost in opposition; the south preceding one is the largest and at the greatest distance. I see also several extremely small stars; but without a regular succession of observations, it is impossible to determine whether any of them may be satellites.

‡ The first satellite was 71° nf; distance 469. The second was $76\frac{1}{3}$° sp; distance 592. They were the observed satellites, and the apparently inverted direction of their motion is evident.

1801, March 8, 12^h 0′. The first satellite is at a great angle south preceding; the second at a great angle north

* The planet being now again past the node, the angular distance of the satellites from zero, and their apparent motions are again inverted, and will remain so.

following; but a line from the second drawn through the planet, leaves the first satellite on the following side.

‡ The first was $85\frac{3}{4}°$ sp; distance 533. The second was $78\frac{1}{4}°$ nf; distance 599.

1801, April 17, 10^h 30′. The first and second satellites are in view at great angles north following the planet. There is a third satellite at a great angle south preceding; in the configuration it is marked exactly in opposition to the second, and at half the distance of the first. Six stars are pointed out.

‡ The first satellite was $77\frac{1}{4}°$ nf; distance 598. The second was 81° nf; distance 586; and the third by the configuration was 81° sp.

1801, April 18, 10° 26′. The first and second satellites are in the configuration at great angles north following. The six stars of last night are in their places, but I do not see any star where the third satellite was marked.

‡ The first satellite was 65° nf; distance 438. The second was 74° nf; distance 578. The third was probably the interior satellite at its greatest southern elongation, which cannot be visible two days together. The configuration of this evening, compared with that of the night before, shows by the situation of the satellites, that their apparent motion is in an inverted direction.

1801, April 19, 10^h 24′. The first satellite was not observed. I saw the second satellite advanced in its orbit in the inverted order. The moon is too bright to make observations on additional satellites.

‡ The first was $7\frac{1}{2}°$ nf; distance 144; invisible. The second was 66° nf; distance 438.

1808, May 27, 10^h 0′. The planet is too low to admit the

use of very high magnifying powers. With 300, however, I have a glimpse of what I suppose to be the two large satellites. A haziness coming on will not permit the angle of position to be taken.

‡ The first was 47° sf: distance 478; the second was 56° np; distance 509. They were therefore both visible.

1809, May 12, 11ʰ 0′. I viewed the planet with 300, but could not perceive the satellites. The planet is too low, and there is a strong twilight.

‡ The first satellite was $72\frac{1}{2}$° sp; distance 578. The second was 53° np; distance 529.

1810, May 25, 10ʰ 40′. I viewed the Georgian planet with the 40 feet telescope, power 400. The disk of it is very bright. Several small stars are near it, but without a series of observations, it cannot be possible to ascertain which of them are satellites. What I suppose to be the second is 65° or 70° nf.

‡ The first was $87\frac{1}{4}$° np; distance 594. The second was 76° np; distance 588. Both satellites were therefore visible, but being among surrounding stars, could not be distinguished from them.

Investigation of several particulars deduced from the foregoing observations, with an exposition of the method by which they have been obtained.

The first use to be made of the numerous angles of position that have been taken, must be an investigation of the place of the node, and the inclination of the orbits of the satellites. When these two particulars are obtained, the times of the periodical

revolutions of the satellites about the planet, may be settled. It will then be necessary to calculate the places of the satellites for the times in which they were observed, in order to identify them; for, notwithstanding all possible care was taken to keep them in view, yet after the long unavoidable annual interruptions, and the periodical interference of the moon, it will be seen, that several mistakes have been made in naming the satellites, which by that means may be easily corrected.

The place of the ascending node, the inclination of the orbits, and the retrograde motions of the satellites determined.

When the observations of the satellites in the year 1797, and the beginning of 1798, are examined, it will be found that the first satellite could seldom be seen, and that its positions, when observed, were always at a great angle from the parallel; the second was also frequently invisible; and its observed positions were likewise at great angles. From these appearances it may be concluded that the planet was approaching to the node of the satellite's orbits. At the latter end of February, and the beginning of March 1798, the position of both the satellites approached to a settled angle, which at last, for two successive days, namely the 11th and 12th of March, remained stationary; I have therefore supposed that the planet was then in its passage through the node, and have in my calculations admitted its place to be five signs, 15 degrees, 30 minutes.

Then a mean of the angles of position 78° 15′,3 and 78° 12′,9 taken the 11th and 12th of March, being 78° 14′ north following, it will appear from the method of calculation

which will be explained, that the inclination of the orbits of the satellites to the ecliptic is 78° 58′.

From these data it also follows, that the motion of the satellites in their revolutions round the planet, by which they are carried from their ascending node to their greatest elongation, is retrograde.

Consideration of the principles by which the periodical revolution of the satellites may be obtained from the observed angles of position.

It will be necessary to premise, that, in order to simplify the investigation of the periodical revolution of the satellites, I have supposed the orbit of the Georgian planet, which differs only about $\frac{3}{4}$ of a degree from the ecliptic, to be coincident with it.

When the rate of the motion of a satellite in its orbit is to be determined from two observed situations, it is required to reduce its first apparent place on the plane of projection, to its real situation in its orbit, I have therefore taken the ascending node for a fixed point, from which we may begin to number the degrees of the satellite's situation; then, as in the second observation, the satellite must also be brought from its apparent place to its real one, we have to allow for three material alterations, that will more or less affect the calculation, according to the length of the interval of time between the two observations.

The first of these alterations is that which takes place in the situation of the parallel, from which the angles are measured; the second is a change in the inclination of the plane of the orbit of the satellite to the plane of projection; and the

third is an alteration in the distance between the ascending node and the extreme point of the transverse axis of the ellipsis, into which the orbit is projected.

In figure 1, (Pl. XVI.) suppose the circle PSFN to represent the plane of the orthographic projection, in which the angles of position are counted from the parallel P and F towards S and N, and expressed by the number of degrees 10, 20, 30 to 90 in each quadrant. Then let there be a moveable circle within the former, of which the degrees should be marked in succession, 10, 20, 30 continued to 360. The inner circle being moveable, the line from 180 to 360 will express, by its different situations, the position of the transverse axis of the ellipsis into which the orbits of the satellites at any given time are projected. The conjugate extending from 90 to 270, and the lines parallel to it, will point out the direction in which the orbits will be more or less contracted according to the different inclinations of their planes to the plane of the projection.

In a triangle PS*p*, figure 2, let P be the pole of the ecliptic. S, a point in the orbit of the satellite, 90 degrees distant from the ascending node. *p*, the point of the greatest northern elongation of the satellites, which on the moveable circle is marked 360. It is the zero of the ellipsis into which the orbits of the satellites are projected, and its calculated situation will regulate the adjustment of the moveable circle. N, a point in a meridional direction, 90 degrees distant from the geocentric place of the planet. The arch PS then being the complement of the inclination of the satellite's orbit to the ecliptic, is therefore given; and the angle at P is equal to the distance of the longitude of the planet from the node. The

angle at S is a right one; and the arch PN, being the measure of the angle of position of that point of the ecliptic where the planet is situated, may be had by Table LIII, published in Dr. Maskelyne's first volume of Observations.

To find what alteration has taken place in the situation of the parallel with regard to the point p, we calculate the position of this point for any given time by the following analogy. (1) cos P : rad : : tan PS : tan Pp; and the difference between PN and Pp will give Np; the complement of which is the position of p, with regard to the parallel. When Pp is greater than PN, the position of P being north following, that of p will be north preceding; but when Pp is less than PN, it will be north following.

To find the inclination of the plane of the satellite's orbit to the plane of projection, we have only to calculate the distance of the poles of these planes; and since the place of the planet is the pole of the projecting plane, and since also the situation of the pole of the orbits is known, which is in longitude 15 degrees 30 minutes of Gemini, and latitude south 11° 2′, therefore in the right angled triangle figure 3, of which GL is the distance of the planet from the longitude of the pole, and PL the given latitude of the pole, we have the analogy (2) rad : cos GL : : cos PL : cos GP; which is the required inclination of the two planes to each other.

To find the distance of the point p, or zero of the projected ellipsis from the ascending node, by figure 2 we have the analogy (3) rad : sin PS : : tan P : tan Sp; and 90° + Sp, before the node, and 90°—Sp, after it, will give the distance of p, or zero point of the inner circle from the ascending node.

The periodical revolutions of the satellites determined.

In the natural order of investigating the motions of the satellites, the first consideration ought to be to identify the observations, lest a star should have been mistaken for a satellite, or one satellite for another; but as the calculations required for this purpose cannot be made without proper tables of their periodical revolutions, I have proceeded in the following manner.

The earliest angles of the positions of the satellites which appeared to be sufficiently accurate for the purpose of settling their motions were taken 1787, February 16, $9^h\ 38'$ mean time. With a wire for the parallel in the focus of the eye-glass, and a magnifying power of 300, the position of the first satellite was five degrees north following; and that of the second was three degrees south following: the motion of the satellites being so near the parallel, there can be no material error in the estimation of the angles; and to prevent the influence of a diversity of errors, I have fixed upon the above-mentioned time as a general epoch to which every calculation of the motion of the satellites has been referred, not only in the determination of the periods, but also in every identification.

With the assistance of the analogies that have been given, seven single periods of the revolution of the first satellite were calculated, from the union of which a general compound period has been deduced. The single periods were calculated from a combination of the observation of the 16th of February 1787, with one of the same year, and with six more of the years 1790, 91, 92, 93, 94 and 96.

In the same manner, nine single periods of the second satellite have been calculated by combining the observation of the 16th of February 1787, with one or more of the succeeding, as far as 1797: the observations of 1798 being too close to the node to give a result that might be depended upon.

It will be proper to give the particulars of one of the single periods, to show what degree of accuracy has been used in the calculation.

By observation, the position of the second satellite, February 16, 9^h 38′ 1787, was 3° sf. The situation of the parallel of declination, by analogy (1) was such that the point F, upon the outer circle of Fig. 1. was opposite to 261° 14′ of the inner circle; 3° sf therefore, was at 258° 14′ of the inner circle. By analogy (2), we then find the inclination of the plane of the orbit to the plane of projection, which enables us, by the argument of the satellites being 78° 14′ from the greatest southern elongation, to reduce the apparent place in the circle to the real one, in the orbit elliptically projected. The correction for this reduction will be + 2° 27′; which being applied, gives 260° 41′. But this being the situation which is numbered from the moveable zero, marked 360, it must be brought to its fixed distance from the ascending node by analogy (3); which gives the distance of the zero from that node for this day 104° 25′; and this being added to the former quantity, gives the real place of the satellite in its orbit from the ascending node 5° 6′.

To combine this with the observation of March 28, 10^h 36′ 1797, when the same satellite was 80° 44′ nf; we find that F, in the parallel should now point to 281° 4′ of the inner circle; and that consequently 80° 44′ on the outer circle, will

be opposite to 1° 48′ of the inner circle; and that to reduce it by the inclination of the orbit, a correction of + 15° 41′ must be applied, which gives its situation in the apparent elliptical orbit 17° 41′ from zero. And when the distance of this point from the ascending node, which now is 91° 6′, is added, we have the satellite's real place in its orbit 108° 47′. Then as in 1787, it was at 5° 6′, and is now at 108° 47′, it must have moved over an arch of 103° 41′ of its orbit, to which, if we add 274 revolutions, we find that the sum of its motion amounts to 98743,68 degrees. The interval of time, in which it has moved over this number of degrees, will be found to be 3693,040277 . . days; from this we obtain the required periodical time, which is $13^{d}\ 11^{h}\ 8'\ 19''$. This single period differs only 40″ from the compound mean period of the revolution of the second satellite.

The seven detached periods of the first satellite, and the nine of the second have all been calculated in the same manner; and in order to obtain a mean value of them, I judged it proper to allow to the duration of every interval of the time, for which they were calculated, its due weight in the scale, by compounding them together. This was done by adding together the single intervals of time in each period, and also adding together the number of degrees passed over in each single period, and computing then the compound period by these collected sums of times and motions, the result of which is, that the first satellite makes a synodical revolution about the planet in $8^{d}\ 16^{h}\ 56'\ 5'',2$, and the second in $13^{d}\ 11^{h}\ 8'\ 59''$.

Explanation of the identifying method.

It is evident, that we cannot be satisfied with a conclusion that is drawn from the apparent situation of an observed satellite, if a doubt should remain whether it actually was the satellite which it is said to be; and where such numerous observations are to be examined, a method of identifying the satellites becomes absolutely necessary.

When the periodical revolution is known, the place where a satellite at any given time should be seen, may be strictly calculated; but a method somewhat less rigorous, and much more expedient, will be sufficient for the purpose; but even this will be found to require tedious computations; for in the first place, the motion of the satellite to be identified for any day, must be cast up by the table of its motion in days, hours, and minutes; and for this purpose, the interval of time for which it is calculated, must first be ascertained; this has been done for every day the satellites have been observed. The amount of the motion in orbit being obtained, it must be added to the number of degrees from which the motion proceeded; this at the already mentioned general epoch of 1787, Feb. 16, 9° 38′, was for the first satellite 11° 27′ from the node, and for the second 5° 6′. The sum then will be the real place of the satellite in its orbit.

Now, to obtain the apparent place of a satellite from a given real one, a table must be made, the first column of which must contain the degree of the geocentric longitude of the planet, for which the rest of the columns are calculated. The three analogies that have been given, are to be used for obtaining the contents of the second, third, and fourth columns;

the fifth, contains the natural sine of the inclination given in the fourth column multiplied by 6.

Such a Table has also been calculated for every degree, from three signs 20° to seven signs 12°, which takes in the whole compass of the observations that have been given. I insert the three first lines of the Table as a sample of its construction.

Geor. longitude of the planet.	Position of 360 or zero.	Distance of zero from ditto.	Inclin. of the plane of the orbits to the plane of projection.	Natural sine of the Inclin. × 6.
3ˢ 20°	79° 27′ np	105° 34′	36° 1′	4,85
21	80 17	105 1	36 57	4,79
22	81 5	104 30	37 54	4,73

In order now to use these preparatory calculations, I made an apparatus consisting of a square piece of pasteboard, upon which a circle was drawn and graduated as in figure 1. To the centre of this, I joined a moveable circle also drawn upon pasteboard, and graduated as in the figure. The radius of the inner circle was exactly six inches, and its circumference was nearly in contact with the inside of the outer circle. From what has already been said of the construction of this figure, its use in the identification of the satellites, will easily be understood by a few examples of it.

With the geocentric longitude of the planet taken from the Nautical Almanack, I take out the required quantities from the different columns of the Table. In this operation it might be sufficient to take only the nearest degree for entering the

Table; but as the difference between any two degrees may be had by inspection, I have always used the nearest half degree. For instance, the quantities for the half degree between three signs 21° and 22°, in the second, third, and fifth columns, will be 80° 41′; 104° 45′; and 4,76. And the same quantities will do for any day from March 7, 1787, till April 23, for which day the quantities must be taken from three signs 22°, &c. &c.

Now, suppose it be required to ascertain whether a satellite called the second, which the 15th of March 1787, at 8^h 7′ was observed to be five degrees south following the planet, was indeed the second satellite? Then I see in the general list of calculated motions, that from February 16, 9^h 38′ to March 15, 8^h 7′, is an interval of 26^d 22^h 29′; in which the second satellite has moved 0° 12′ from its place; and as it was then at 5° 6′, it is therefore now 5° 18′ from the ascending node of its orbit.

In using the identifying apparatus, the first thing to be done is, the adjustment of the inner circle to the position it ought to have for the day of observation, which is pointed out by the geocentric longitude of the planet three signs $21\frac{1}{2}$ degrees; the zero must consequently be adjusted to 80° 41′ north preceding; I therefore turn the inner circle upon its centre, till the point 360 is opposite to $80\frac{2}{3}$° np; for in the adjustment of the circles to each other, and in reading off the angles pointed out by them, a critical estimation of minutes has not been attempted; whenever, therefore, minutes are given, they must be understood to relate to calculations, or to measures taken with a micrometer.

In the next place, the point of the inner circle, answering

to the calculated situation of the satellite in its orbit, is to be found by the tabular quantity 104° 45′, which is the distance of the ascending node from the zero of that circle; and as the satellite is 5° 18′ from the same node, the quantity given by the Table must be deducted from the same; that is from 365° 18′, and the remainder 260° 33′ will be the place of the satellite on the moveable circle.

Finally, to get the angle of position at which the satellite will be seen, the two ends of a proportional compass must be adjusted to each other, so that when one end of it is opened to six inches, the other may give the quantity in the last column of the Table, which in the present case is 4,76 inches. The distance from $260\frac{1}{2}$° to the transverse must then be measured by the long end of the compass, in a direction parallel to the lines in the figure; and the opening of the short end must be set back again in the same direction, from the transverse towards $260\frac{1}{2}$°; a fine point must be marked with the end of it upon the pasteboard. A black silk thread fixed to the centre of the circle, may then be stretched over the impressed point to intersect the degrees of the outer circle, upon which the positions are reckoned in the order they are marked; and this being done, it will be seen that the intersection of the thread falls upon $2\frac{1}{4}$ degrees of the south following quadrant; which sufficiently identifies the satellite.

In observations that are made when we are in, or very near the node of the orbits of the satellites, their angular positions undergo hardly any change, and can therefore be of no use for identifying them; but they may then be distinguished by their proportional distances from the planet; and these may be very conveniently had in six hundredth parts

of the respective radii of the satellites, and the impression of the fine point whereby the angle of position is obtained will be of eminent use; for by putting one leg of the compass upon this point, and extending the other to the centre of the circle, we shall in the present case have 4,81 inches for the measure of the required distance, which as the radius of the circle is six inches, will be $\frac{481}{600}$ parts of it; and in such parts all the distances which are given in the foregoing observations have been expressed.

I have called this manner of obtaining the angles of position and proportional distances, the identifying method, that it may remain distinguished from strict computation; there is, however, so much real calculation mixed with it, that I may confidently draw the following interesting conclusions from it.

I. With the light of my 20 feet telescope, the first satellite generally becomes invisible at the distance of a little more than half its greatest elongation; I suppose it to be when the identified measure of it is from 302 to about 310.

II. The second satellite becomes invisible at very nearly half the distance of its greatest elongation; I suppose it to be when its identified distance is from 295 to about 305.

III. An interior satellite as large as the first, must be more than half the greatest elongation of the first satellite from the planet; and if it be smaller, it must be at so much greater a distance from the planet, to be seen at its greatest elongation. Nor can there be any chance for seeing it two nights together, when the orbits are contracted by projection.

IV. Exterior satellites that are very faint when at their greatest elongation, can hardly ever be seen at any other time when the orbits are contracted.

V. The first satellite is probably larger than the second; for though the latter is generally the brightest, it seems to be only in consequence of its being farthest from the planet. On comparing the limit of its disappearance with the number 302, expressing that at which the first satellite generally ceases to be visible, we find that the second satellite, upon its own scale, should not be lost in the light of the planet till it came within the limit of 224, instead of 295.

VI. Both the satellites are subject to great variations of light, not owing to the changeable clearness of the air at different times; for by comparing the brightness of one satellite with that of the other when they are seen together, the state of the air will be of equal clearness to both, and yet their comparative brightness has been observed to be very different: for instance, March 14, 1793, the first satellite was brighter than the second, when the distance of the former was to that of the latter as 172 to 235; and February 26, 1798, the first was small, and the second larger when the distance of the former was to that of the latter as 175 to 210.

VII. The variable brightness of the satellites may be owing to a rotation upon their axes, whereby they alternately present different parts of their surfaces to our view. These variations may also arise from their having atmospheres that occasionally hide or expose the dark surface of their bodies, as is the case with the sun, Jupiter and Saturn.

VIII. The real angular distances of the satellites from the planet may be determined from the measures that are given in

the observations, but to enter critically into this subject would extend far beyond the compass of this Paper. The disagreement of the measures is very considerable; this will, however, not appear so remarkable, when the faintness of the satellites is considered, which will not admit of an illumination of the wires of the micrometer. The two measures of the distance of the first satellite that were taken Oct. 11, 1787, and March 12, 1798, should both be considered; but a selection of about six of the most consistent measures of the second satellite, will probably be necessary to give the truest result. For instance, those of 1787, March 18, 20, April 9, and November 9, with those of 1798, March 12 and 19, might be taken. If these measures are brought to their greatest elongation by the identified distances that are given with them, some kind of judgment may be formed of the probable result, when calculation is applied to them.

In my observations I have supposed the distances of the first and second satellites to be 36 and 48 seconds, and by this proportion I have occasionally reduced the identified distances of the two satellites to an equal scale.

IX. The existence of additional satellites has already been considered in a former paper.* Many remarks on them were given under the four heads of interior, intermediate, exterior, and more distant satellites; and, as many additions are contained in the foregoing observations, I shall review the former remarks, with the assistance of the light which the identifying method has thrown upon them, and afterwards, in the same order consider, in each class, what evidence of the existence of such satellites may be derived from the additional observations, especially from those that were made in the year

* Phil. Trans. for 1798, page 59.

1798, when the orbits of the satellites were contracted into a line, which might be examined with greater facility than a more expanded space; and where even the very situation of a star in this given direction, rather than in the numberless others, in which it might be placed, must be a presumption of its being a satellite, provided its distance at the same time should not exceed a certain probable limit.

An interior satellite.

The supposed interior satellite observed January 18, 1790, could not be a star, whose existence was doubtful, as it had light enough for an estimation of its distance in diameters of the planet; its absence, however, not having been noticed the 19th of January, although great attention has been shown to ascertain the sidereal nature of another supposed satellite, observed at the same time with the former, leaves the observation of the eighteenth unsupported.

The observation of the 4th of March 1794, which has been supposed to relate to an interior satellite, is by the identifying method proved to belong to the second satellite; and its observed absence on the 5th from the place where it was the 4th, being thus verified and accounted for, shows that great confidence may be placed on such observations.

The observation of an interior satellite of the 27th of March, 1794, is without a subsequent observation; but then it has already been noticed in remark III, that an interior satellite cannot be seen two successive days, when its orbit is already contracted, as it was on the day of observation.

Addition.

The 15th of February, 1798, an interior satellite was seen about its greatest northern elongation; as it was between the planet and the second satellite (miscalled the fifth), its position must have been 84° 49′ nf. On account of its faintness it was not seen immediately, but as soon as it was perceived, it was surmised to be the second satellite; but the identified distance of the second, which was 455, is much too far for the observed distance of the faint satellite, and proves that in reality the supposed fifth was the second satellite. This is moreover confirmed by its brightness, and by the angle of position which was taken. The first satellite was invisible, its distance being only 124; it even remained invisible the next day, when its distance was 289; the observed faint one, therefore, must have been an interior satellite in a distant part of its northern elongation. The 16th of February, the place where the satellite had been the day before was scrupulously examined in looking for the supposed fifth, and as there was no star remaining in that place, the removal of the interior satellite from its former situation was thereby also ascertained. It has already been noticed that in the contracted position of the orbits an interior satellite observed the 15th could not possibly be seen the 16th, which accounts for its not being noticed the last day of observation. This is one of the cases where the singular situation of a star alone, is almost sufficient to prove it to be a satellite.

The 17th of April, 1801, the interior satellite, which had been seen in its greatest northern elongation, was now seen about its greatest southern elongation. Its situation, by

identification, was 81° sp. The 18th of April, when the stars that were pointed out were looked after, and were all found remaining in their places, no star could be seen where the interior satellite had been situated the 17th; nor could it be expected to be visible, as, by its motion towards the planet, it must already have been involved in the splendour of its light.

An intermediate satellite.

The 26th of March, 1794, an intermediate satellite was seen; by the configuration its distance was greater than that of the first, but less than that of the second. By identification its situation must have been $59\frac{1}{2}°$ nf. The 27th of March, the satellite was no longer in the place where it had been seen the 26th, and moreover, a very small star was seen in a place that agreed with what would be the situation of an intermediate satellite, had it accompanied the planet.

An exterior satellite.

The 9th of February, 1790, an exterior satellite was observed. It was by the configuration at double the distance of the second satellite, and by identification, its position was $61\frac{1}{2}°$ sf. The observations of two succeeding days proved, that it remained no longer in the place where it had been seen on the 9th.

The 27th of March, 1794, some distant stars south of the planet were observed as being supposed satellites; but they are not sufficiently supported by succeeding observations.

The 5th of March 1796, a star was seen, which the night before had not been in the place where it was at the time of the observation. By the configuration its identified place

must have been 72° np; and its distance exceeded that of the second satellite.

Addition.

The 31st of January, 1791, a satellite in opposition to the second, and at about double the distance from the planet was observed; its identified position was $78\frac{1}{2}$° np. The 2d of February all the stars of the configuration that had been pointed out, were seen remaining in their places, but the exterior satellite was not among them.

The 26th of February, 1792, a star at double the distance of the first satellite was pointed out, but it has not been accounted for in succeeding observations. By remark IV, however, faint exterior satellites can hardly be expected to be seen at any other time, than when they are about their greatest elongation.

The 11th of February, 1798, an exterior satellite, called the fifth, was observed; its situation was measured 89° 19',5 nf. The 13th of February, this satellite was no longer in the place where it had been seen the 11th. The 15th of February it was again ascertained that the satellite had left its former situation. The orbits of the satellites, at the time of these observations, were already contracted into a line, and a very faint satellite like this could not remain visible two, and four days successively; its motion, according to remark IV, would in a short time immerge it again into the effusive light of the planet, and render it invisible.

More distant satellites.

The 28th of February, 1794, a small star was seen in a place where the 26th there was none. By the configuration

of that day, and the identifying method, it was at a considerable distance, about 24 degrees north following the planet, and not far from a lettered star which was smaller than the new star. It cannot be supposed that a larger star should have been omitted to have been marked in the situation pointed out by smaller lettered stars, where it must have been seen the 26th, if it had been there.

The 27th of March, 1794, south of the planet, at a considerable distance, were small stars, that had the appearance of satellites; but there are no subsequent observations of them.

The 28th of March, 1797, a distant star is mentioned that was not seen the 25th, although the situation of the lettered stars of that day was carefully examined.

Addition.

The 16th of February, 1798, at $11^h\ 12'$ a very faint satellite, called the sixth, was observed, and from its distance, supposed to be a little before or after its greatest southern elongation. It was so faint, that a small alteration in the clearness of the air, rendered it invisible. On the 18th the sixth satellite was seen again, and, being nearer the planet than it was on the 16th at $11^h\ 12'$, it was supposed to be on its return from the greatest southern elongation. It was also ascertained on the 18th, that it had left the place where it was seen on the 16th. The angle of its position, by a measure taken of it, was $82°\ 55'$ south preceding.

Fig. 1.

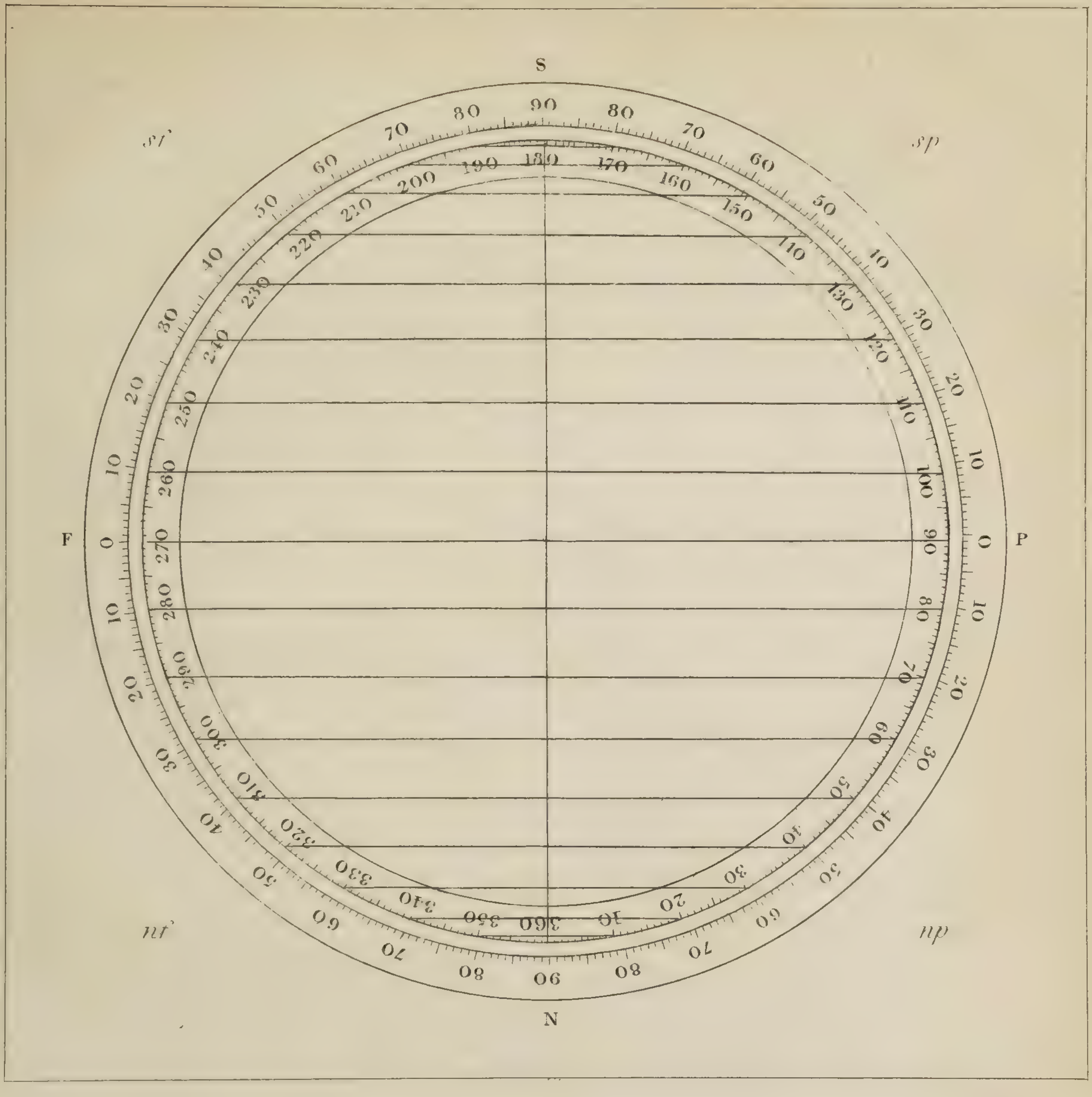


Fig. 2.

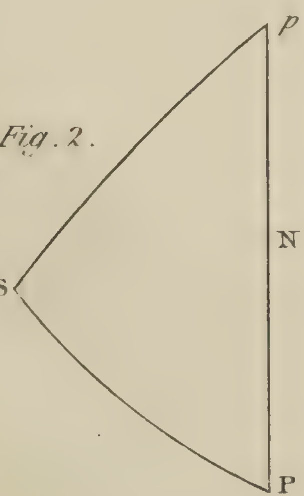
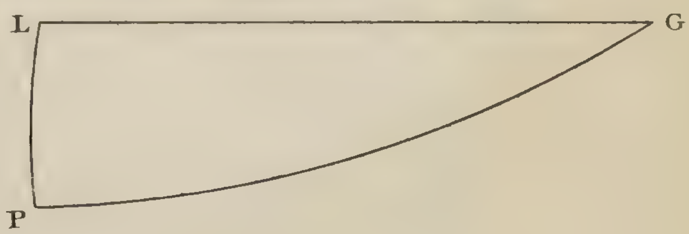


Fig. 3.



Jas Basire, sculp.

XX. *An account of some experiments with a large voltaic battery.* *By* J. G. Children, *Esq. F. R. S.*

Read June 15, 1815.

In 1809 I presented to the Society a short account of some experiments performed with a voltaic battery of unusually large plates, which has been honoured by publication in the Philosophical Transactions for that year. Since that period I have constructed another of still larger dimensions, the effects of which form the subject of the present communication. The copper and zinc plates of this apparatus are connected together, in the usual order, by leaden straps; they are 6 feet long, by 2 feet 8 inches broad, each plate presenting 32 square feet of surface. All the plates are attached to a strong wooden frame suspended by ropes and pullies, which being balanced by counterpoises, is easily lowered and elevated, so as to immerse the plates in the acid, or raise them out of it, at pleasure. The first trials of the power of this instrument were made in July 1813, in the presence of several philosophical friends, but the effects then fell very short of my expectations, arising, as I afterwards found, from a defect in the construction, which has been since remedied, and another copper plate added to each member of the series, so that every cell now contains one zinc and two copper plates, and each surface of zinc is opposed to a surface of copper. This was done at the suggestion of Dr. Wollaston, and has very

considerably increased the power of the battery. From some comparative experiments, which I made with a small apparatus, the increase in quantity of electricity, thus effected, is at least one half. The cells of the battery are 21 in number, and their united capacities amount to 945 gallons. To each pole of the battery a leaden pipe, about $\frac{3}{4}$ths of an inch in diameter, is attached by solder, and the opposite end of each pipe immersed in a basin of mercury, (a separate basin for each pipe,) by means of which the circuit is compleated, and a perfect contact ensured. The first experiments I shall mention were made on the comparative facility with which different metals are ignited when placed in the electrical circuit. For this purpose, in each experiment, two wires of dissimilar metals were taken, of equal diameter and length; one end of each was in contact with one of the basins of mercury communicating with the poles of the battery, and the other end bent to an angle, and the wires connected continuously by hooking them together. The length of each wire was 8 inches, and the diameter $\frac{1}{30}$th of an inch. The battery was moderately excited by a charge of 1 part acid diluted with 40 parts of water.

Exp. 1. A platina and a gold wire being thus connected, and introduced into the electrical circuit, the platina was instantly ignited, the gold remained unaffected.

Exp. 2. A similar arrangement of gold and silver wires. The gold was ignited, the silver not.

Exp. 3. The same with gold and copper. No perceptible difference in the state of ignition; both metals were heated red.

Exp. 4. Gold and iron. The iron was ignited; the gold unchanged.

Exp. 5. Platina and iron. The iron ignited instantly at the point of contact next the pole of the battery. Then the platina became ignited through its whole extent. After this the iron became more intensely heated than the platina, and the ignition of the latter decreased.

Exp. 6. Platina and zinc. The platina was ignited: the zinc was not; but melted at the point of contact. In a subsequent experiment, the zinc did not melt; but the platina ignited as before.

Exp. 7. Zinc and iron. The iron was ignited: the zinc bore the heat without fusing.

Exp. 8. Lead and platina. The lead fused at the point of contact.

Exp. 9. Tin and platina. The tin fused at the point of contact. No ignition of either wire took place in the two last experiments.

Exp. 10. Zinc and silver. The zinc was ignited before it melted: the silver was not ignited.

The results in every case were the same to whichever pole of the battery either wire was presented. I varied these experiments by introducing several alternations of different wires continuously connected, into the circuit, and obtained in every instance analogous results. Thus

Exp. 11. Alternations of platina and silver, three times repeated: all the platina wires were ignited, and none of the silver.

Exp. 12. One zinc wire between two platina: both the platina wires were ignited, the zinc not.

Exp. 13. One iron between two platina. Both the latter first ignited; then the iron, which soon became most heated, and fused.

It is unnecessary to enter into a farther detail of these experiments; it will be sufficient to say generally, that when wires of several different metals were introduced at once into the circuit, the order of their ignition was precisely that of the former experiments. In one experiment with copper and gold, the copper was decidedly most heated.

I feel some difficulty in attempting an explanation of the preceding phenomena, and offer the following conjecture with diffidence. When a perfect communication is established between the poles of the battery, the electricity circulates without producing any visible effect; but if it meet with resistance in its passage, it manifests itself by chemical action, by the evolution of heat, or both. Thus, if a bar of metal be connected with one pole of the battery, and its extremity immersed in a basin of mercury connected with the other pole, at the instant the surfaces come in contact, heat and light are evolved, which cease as soon as the bar, if it be of sufficient size, is fairly plunged beneath the surface of the quicksilver. If the circuit be completed by two pieces of charcoal, the evolution of heat and light is permanent, as long as their surfaces remain in contact, because that contact can never be so perfect, as to oppose no resistance to the electricity; whereas, in the case of the bar of metal and the mercury, it soon becomes complete, and the current is then uninterrupted. Resistance, therefore, appears to occasion the developement of heat, (whatever be the ultimate cause of the phenomenon,) and as this must be inversely as the conducting power, when any two of the wires connected continuously are placed in the circuit, that which is the worst conductor must be most heated; and thus platina, having the lowest conducting power, is ignited before all the rest; and silver, which

conducts best, is not heated red when connected with any of the other metals. Should it be objected, that if the electricity meet with greater resistance in one body than in the other, equal quantities cannot be transmitted in equal times by the two substances, (a circumstance essential to electrical action,) I answer, that a body may be propelled through two media of different densities, with equal velocity, if the propelling forces be proportionate to the resistances; and it is a necessary consequence that whatever effect the passage of the body be capable of producing in the least resisting medium, it will produce it in a still greater degree in the most resisting; and if that effect be heat, the greatest portion will be developed in the latter instance. In the case in question, indeed, there is but one propelling force; but as it is sufficient to overcome the greater resistance, the analogy is unshaken. That the ignition of the wire is generally first perceptible at the point of contact next the pole of the battery (to whichever pole it be presented) is in favour of the hypothesis. I once thought the phenomena might be owing to the joint effect of difference of conducting power, and inequality of the capacity of different metals for heat; but the experiments of Crawford, Leslie, Dalton, Irvine, and others, militate against that idea; for, according to them, the capacities of iron and platina exceed those of all the other metals, whereas, on the supposition alluded to, they ought to be inferior. From the foregoing results, the order of the conducting powers of the metals tried is silver, zinc, gold, copper, iron, and platina. Tin and lead fuse so immediately at the point of contact, that they cannot be placed. Between gold and copper the difference is trifling; and with regard to platina and iron, their

relations to each other, in this circumstance, seem to be affected by elevation of temperature. It may be observed, that the order of the above metals, as conductors of electricity, nearly follows that of their powers to conduct heat.

In an experiment in which equal lengths of two platina wires, of unequal diameter, (the larger being $\frac{1}{30}$, the smaller $\frac{1}{50}$ of an inch,) were placed together in the circuit *parallel* to each other, the thicker wire was ignited, because it conveyed more electricity without proportional increase of cooling surface. When connected continuously, the order of ignition was reversed. These two results were foreseen by Dr. WOLLASTON, who suggested the experiments.

The experiments which I now proceed to mention, were made with the battery in a high state of excitation; and I consider them as representing nearly the maximum of effect which it is capable of producing. As the quantity of acid was increased from time to time, and that previously added often almost spent before fresh was put in, it is not easy to say exactly what proportion it bore to the water; perhaps the largest may be stated at about $\frac{1}{20}$th. On this, as on former occasions, I found a mixture of nitrous and sulphuric acids, to produce the most powerful and permanent effects.

Exp. 1. 5 ft. 6 in. of platina wire, $\frac{11}{100}$ of an inch in diameter, were heated red throughout, visible in full daylight.

Exp. 2. 8 ft. 6 in. of platina wire, $\frac{44}{100}$ of an inch in diameter, were heated red.

Exp. 3. A bar of platina $\frac{1}{6}$ of an inch square, and $2\frac{1}{4}$ inches long, was also heated red, and fused at the end; and,

Exp. 4, a round bar of the same metal, $\frac{276}{1000}$ of an inch

in diameter, and $2\frac{1}{2}$ inches in length, was heated bright red throughout.

Exp. 5. Fine points of boxwood charcoal intensely ignited in chlorine, neither suffered any change, nor produced any in the gas. The result was similar when heated in azote.

I next tried the power of the battery to fuse several refractory substances. The subject of experiment was placed in a small cavity, made in a piece of well burnt boxwood charcoal, floating on the surface of the mercury in one of the basins before mentioned, and the circuit completed by another piece of charcoal, communicating by stout copper wire, with the other basin.

Exp. 1. Oxide of tungsten, which, (as well as all the other metallic oxides operated on,) had been previously intensely ignited in a charcoal crucible, in a powerful furnace, fused, and was partially reduced. The metal greyish white, heavy, brilliant, and very brittle.

Exp. 2. Oxide of tantalum. A very small portion fused. The grains have a reddish yellow colour, and are extremely brittle.

Exp. 3. Oxide of uranium; fused, but not reduced.

Exp. 4. Oxide of titanium; fused, not reduced. When intensely heated it burnt, throwing off brilliant sparks like iron.

Exp. 5. Oxide of cerium; fused, and when intensely heated it burnt with a large, vivid, white flame, and was partly volatilized, but not reduced. The fused oxide, on exposure for a few hours to the air, fell into a light brown powder, containing numerous shining particles of a silvery lustre,

interspersed amongst it, and exhaled an odour, similar to that of phosphuretted hydrogene.

Exp. 6. Oxide of molybdena; readily fused and reduced. The metal is very brittle, of a steel grey colour, and soon becomes covered with a thin coat of purple oxide.

Exp. 7. Compound ore of iridium and osmium; fused into a globule.

Exp. 8. Pure iridium; fused into an imperfect globule, not quite free from small cavities, and weighing 7.1 grains. The metal is white, very brilliant, and in its present state its specific gravity is 18,68, which must be much too low, on account of the porous state of the globule. In the minutes of the experiments, in July 1813, mention is made of the fusion of a small portion of pure iridium into a globule weighing $\frac{62}{100}$ of a grain, which had been previously submitted to the action of a battery of 2000 plates, of four inches square, without melting.

Exp. 9. Ruby and sapphire, were not fused.

Exp. 10. Blue spinel, ran into a slag.

Exp. 11. Gadolinite, fused into a globule.

Exp. 12. Magnesia, was agglutinated.

Exp. 13. Zircon from Norway, was imperfectly fused.

Exp. 14. Quartz, silex, and plumbago, were not affected.

In the year 1796, M. CLOUET converted iron into steel, by cementation with the diamond, with the view of confirming the nature of that substance, and of ascertaining the exact state in which carbon exists in steel. CLOUET had also previously formed steel by cementation with carbonate of lime. Mr. MUSHET repeated this experiment, using instead of the

carbonate, caustic lime, and obtained also what he considered to be cast steel; whence he concluded that the carbon necessary to convert the iron into steel had not been furnished, as Clouet supposed, by decomposition of the carbonic acid, but that it had found its way from the ignited gas of the furnace to the iron. This result occasioned suspicions of the accuracy of the deductions from the experiment with the diamond; and Mr. Mushet accordingly, at the suggestion of the editor of the Philosophical Magazine, repeated the experiment made at the Polytechnic School, only *keeping out the diamond.* The results (for he made several experiments) uniformly gave him good cast steel, whence he concludes that we are still without any satisfactory or conclusive proof of the steelification of iron solely by means of the diamond; and adds that he doubts whether the diamond afforded *even one particle of carbon to the iron.* The details of both Clouet's and Mushet's experiments, may be found in the fifth volume of the Philosophical Magazine. Sir George M'Kenzie repeated both Clouet's experiments and those of Mr. Mushet, and obtained results confirming the conclusions of the French chemist. The labours of this gentleman indeed seem sufficiently conclusive; but, if a doubt should remain, it occurred to Mr. Pepys, that the battery would afford an *experimentum crucis* on the subject; and his ingenuity readily suggested a mode of making it, every way unobjectionable. He bent a wire of pure soft iron, so as to form an angle in the middle, in which part he divided it longitudinally, by a fine saw. In the opening so formed, he placed diamond powder, securing it in its situation by two finer wires, laid above and below it, and kept from shifting, by another small wire, bound firmly and

closely round them. All the wires were of pure soft iron, and the part containing the diamond powder, was enveloped by thin leaves of talc. Thus arranged, the apparatus was placed in the electrical circuit, when it soon became red hot, and was kept so for six minutes. The ignition was so far from intense, that few who witnessed the experiment, expected, I believe, any decisive result. On opening the wire, however, Mr. PEPYS found that the whole of the diamond had disappeared; the interior surface of the iron had fused into numerous cavities, notwithstanding the very moderate heat to which it had been exposed; and all that part which had been in contact with the diamond was converted into perfect blistered steel. A portion of it being heated red and plunged into water, became so hard as to resist the file, and to scratch glass. This result is conclusive, for as the contact of any carbonaceous substance, except the included diamond, was effectually guarded against, to that alone can the change produced in the iron be referred. This experiment will also probably be deemed fatal to the opinion of those mineralogists (if any do still maintain that opinion,) who class the diamond with substances of the siliceous genus.

When dry caustic potash was exposed to the intense heat between the two pieces of charcoal, it fused, and appeared to decompose, throwing off a large flame of the peculiar purple red colour, that attends the combustion of potassium. When moist caustic potash was placed in the circuit, the water only was decomposed.

I endeavoured to ascertain if there be any difference in the degree of heat produced at either pole of the battery, by placing two small earthen-ware cups, each containing an equal

weight of mercury, in the circuit, and connected together by a platina wire of such size and length as to be kept constantly ignited. The mercury in the cup connected with the zinc end of the battery, attained in 20 minutes the temperature of 121°; that in the other cup 112°.

The battery, even in its most active state, communicated no charge to the Leyden phial.

I give the following experiment, the last with which I shall occupy the time of the Society, without comment. I separated all the zinc from the copper plates, by dividing the leaden straps that united them; and then by means of other leaden straps, I connected all the zinc plates together as one plate, and all the copper plates in the same manner; thus reducing the whole battery to only two plates, each presenting a surface of 1344 square feet, reckoning the copper surface as only equal to the zinc. When the plates, thus arranged, were suspended, quite out of contact with the acid, a communication was made between the two metallic surfaces by means of a platina wire $\frac{1}{5000}$th of an inch diameter, and about $\frac{1}{30}$th of an inch long, with every possible attention to ensure a perfect contact; but, although the experiment was made in the dark, not the slightest appearance of ignition was perceptible in the minute wire by which these extensive surfaces were connected. It is known, I believe, to almost every member of this society, that Dr. Wollaston has shown, with the delicate apparatus invented by him, that a platina wire, of the same dimensions as that just mentioned, is instantly ignited by a single pair of plates one inch square, on being immersed in a diluted acid. The ratio of the areas of the plates of the respective batteries is as 1 to 48384. When the

plates of the large battery, in the usual order of arrangement, were immersed in mere pump water, previous to any acid having been put into the cells, they ignited $\frac{1}{4}$th of an inch of platina wire $\frac{1}{200}$th of an inch diameter, and fused the end of it into a perfect globule.

XXI. *On the dispersive power of the atmosphere, and its effect on astronomical observations. By* Stephen Lee, *Clerk and Librarian to the Royal Society. Communicated by* W. H. Wollaston, *M. D. Sec. R. S.*

Read June 15, 1815.

NOTWITHSTANDING the pains which astronomers have taken to determine accurately the refraction of mixed light, nothing, I believe, has ever been done towards ascertaining the dispersive power of common air, or comparative degree of refrangibility of the differently coloured rays in their passage through our atmosphere.

The importance of such an inquiry, however, must be obvious to every one who duly considers the effect which the different degrees of refrangibility of the variously coloured lights must necessarily produce in the apparent situations of differently coloured objects. Stars of different colours must be differently refracted, and the apparent altitude of the sun must vary according to the colour of the dark glass through which he is viewed.

Perhaps this cause alone is sufficient to explain the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that deduced from observations of the sun during the solstices, which has so long occupied the attention of astronomers, and has never yet been satisfactorily accounted for.*

* Vide Mr. PIAZZI's Memoir on the Obliquity of the Ecliptic, in the Memoirs of the Società Italiana, Vol. XI.

The dispersive power of the atmosphere will also show why Aldebaran and the red stars are sometimes seen projected on the moon's disk in occultations by that planet, especially when the immersion or emersion happens to be near her upper limb. For the light of the moon being white, is more refracted than that of the star, and consequently her limb more elevated, which would occasion the star to appear within her disk a few seconds before or after contact.†

The great disagreement which is found to exist in the declination of several of the fixed stars, as given by different observers, may probably be traced to the same cause, stars being more or less refracted according to the predominant colour of which their light is composed.

That the fixed stars differ from each other in respect to the composition of their light, must be obvious to any one who will only take the trouble of comparing them on a fine night. They present a striking variety of colour even to the naked eye. But this difference becomes still more perceptible when they are viewed through a prism properly adapted to the eye-piece of a reflecting telescope.

A star viewed in this manner is converted into a prismatic spectrum. Sirius and the *brilliant* white stars exhibit a large brush of beautiful violet, and the most refrangible colours in great abundance. Aldebaran, α Orionis, and the red stars show only a small proportion of those colours, whilst the dull white stars exhibit a great quantity of intense green light.

† Vide Philosophical Transactions, Vol. LXXXIV. p. 345. Histoire Céleste Françoise, Tome I. p. 393, 403, 413, 425, 428, 467, and Connoissance des Temps for 1817.

The planets also differ much from each other in this respect. The moon, Venus, and Jupiter, seem to possess every colour; but the green is very pale in all of them. Mercury and Mars appear deficient in the middle and most refrangible rays, whilst the light of Saturn seems to be composed principally of the mean rays with a very small proportion of the extreme colours of the prism.*

The different refrangibility of the differently coloured rays is very visible in stars near the horizon. If viewed on a fine night with a power of 200 and upwards, they appear expanded into a prismatic spectrum. Sirius, when within a few degrees of the horizon, presents a most beautiful object.

Having remarked the very oblong figure which the spectrum assumes when near the horizon, and found from repeated observations of different stars that the separation of light begins to be visible as high as 40° or 50° of altitude, I was led to believe that the dispersive power of the atmosphere must be sufficient, in many cases, to produce considerable effect on astronomical observations; and, consequently, to suppose that it would be desirable to ascertain, if possible, the exact degree of separation of the several rays.†

With this view, therefore, I began a series of observations;

* Query. May not this circumstance explain why Saturn, though less brilliant, bears magnifying better than Jupiter and Venus?

† Dr. Herschel, in a note to his Paper on Double Stars, published in the seventy-fifth volume of the Philosophical Transactions, says that the prismatic power of the atmosphere is very visible in low stars; and very justly observes that this power ought not to be overlooked in delicate and low observations: he gives the measure of two diameters of ε Sagittarii, which seem to indicate that the refraction of the extreme rays is about $\frac{1}{94}\pm$, the mean refraction. I think it due to that great astronomer to mention the circumstance, though it was totally unknown to me till long after I had completed my observations on Mars.

the result of which, and the manner of conducting them, I shall now take the liberty of laying before the Society.

The first instrument employed for the purpose with any degree of satisfaction, was the two feet reflector made by Mr. Short, and which belongs to the Royal Society. In the compound focus of the eye piece of this telescope, I fixed *horizontally* a narrow slip of ivory. With the instrument thus prepared, I observed Capella, and other low stars near the meridian. By carefully noticing the intervals of time between the first contact and total immersion, and between the first appearance and complete emersion of the star from behind the slip of ivory, I obtained data from which it was easy to calculate its vertical breadth, which, compared by estimation with its horizontal breadth, gave the separation of the extreme rays of light.

It was impossible, however, to remain long satisfied with such coarse measures, and not finding it convenient to go to much expense on this account, I applied to my friend Mr. Rennie for the loan of his seven feet reflector made by Dr. Herschel, to which I adapted a very excellent wire micrometer made by Mr. Troughton; and thus, by the kind assistance of my friends, I obtained instruments capable of measuring small angles to the fraction of a second of space.

With this apparatus I repeatedly measured the diameter of Mars during his opposition in 1813. The Society's apartments being well situated for the purpose, I observed the planet as soon as he became visible over the buildings, until he attained his meridian altitude, which never exceeded 15°.

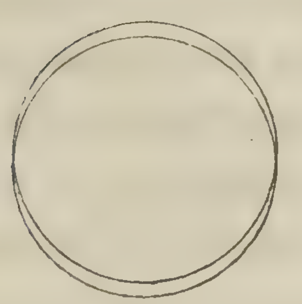
With a power of 170 and upwards, the disk of the planet appeared much elongated, especially when near the horizon; the upper limb was of a fine blue, the lower limb of a deep red.

By carefully measuring the diameter of Mars and the breadth of the coloured edges, I endeavoured to ascertain, as exactly as possible, the degree of separation of the differently coloured images of the planet.

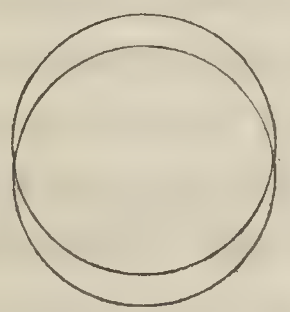
But after all it was no easy matter to measure the coloured edges exactly, for the light which was necessary to illuminate the wires, rendered the colours so very faint as to make it extremely difficult to distinguish their precise boundaries. For this reason, and because I wished to apply higher powers than could be used with the micrometer, I adopted the following method, which I found far more convenient, and is, I believe, quite as accurate.

I drew on a sheet of paper several figures of two equal circles cutting each other, placing the centres of the circles in the first figure $\frac{1}{10}$ of their radius from each other; in the second figure $\frac{2}{10}$; in the third $\frac{3}{10}$; and so on. The upper crescent of these figures I painted blue, the lower crescent red, and the part common to both circles of a reddish yellow, softening the colours into each other as they appeared in the planet. For I considered that, in fact, it was not a single image of Mars that was seen, but a number of differently coloured images, lying in the same direction, though lifted one above another, as represented in the annexed figures.

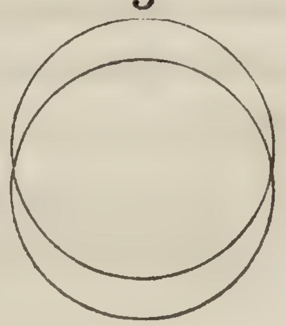
1



2



3



Having prepared a number of these drawings, I repeatedly compared them with the planet viewed through the telescope with different magnifying powers, carefully noting which figure he most resembled, and the time of observation.

This being done, it was easy to calculate the exact altitude from the time of observation, and to make a very near estimate of the separation of the images from the figure referred to, compared with the diameter of the planet found by the micrometer.

From a great number of observations on Mars, Venus, and the fixed stars, taken in all these different ways, I found the deviation of the extreme rays of light to be between $\frac{1}{60}$ and $\frac{1}{70}$ part of the total refraction.

It has already been observed, that the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that from observations of the sun, may perhaps be traced to the use of dark glasses. But this will appear more evidently from a reference to the method employed by Dr. Bradley for determining the quantity of refraction, which method is very clearly described by Dr. Maskelyne in the seventy-seventh volume of the Philosophical Transactions. He says,

That Dr. Bradley got the height of the pole from observations of the circumpolar stars, and the height of the equator from observations of the sun at the two equinoxes; that he found these two altitudes together amounted to 89°, 58′, 3″, which being subtracted from 90°, leaves 1′, 57″, for the sum of the refractions at the pole and equator; and that of this quantity he assigned $45\frac{1}{2}''$ to the former, and $71\frac{1}{2}''$ to the latter.

But Dr. BRADLEY undoubtedly made use of dark glasses for observing the sun, probably smoked glasses, which would give him a pale orange coloured image, or one of less than mean refrangibility; consequently, the quantity of refraction as found by Dr. BRADLEY must be too small for white light.

This alone is sufficient to produce a small difference between the results of our observations of the sun and of the stars. I shall now mention two other circumstances which appear to me to have produced a still greater apparent disagreement.

The publication of the Nautical Almanac in 1767, led to the general use of HADLEY'S sextant. In the construction of this instrument, coloured glasses were indispensibly necessary; and the great convenience in the use of them over smoked glasses, soon occasioned the application of them to all other instruments. These glasses generally give a deep red image, or one of less refrangibility than smoked glass. The effect of this alteration, therefore, should have been, that arising from too great correction for refraction in every thing depending on observations of the sun.

The introduction of achromatic object glasses * produced an error of a different kind; and one which, in certain cases, tends to correct the other. In the single object glass telescope (and there were no others in BRADLEY'S time) the differently coloured images are formed at different focal distances, which, in a manner, compels the observer to adjust his instrument to the most intense light; that is to say, to the orange coloured † image; by this means the fainter colours,

* An achromatic object glass was first applied to the south quadrant at Greenwich in 1772, and to the north quadrant in 1789.

† Vide Newton's Optics, Book I. Part I. Prop. VII.

which occupy the greatest space in the spectrum,* are dissipated, and lost among the more powerful rays. In good achromatic telescopes the case is very different, for all the rays being collected by them into one point, every colour is seen in its proper place, so that the observer, in bisecting the spectrum, takes the altitude of the mean, or the upper extremity of the green image.

But if the upper extremity of the green image be taken in observations of circumpolar stars, a greater correction than Dr. Bradley's ought to be applied, in order to get the true height of the pole.

It may not be amiss to observe here, that the observations of Mr. Lalande at Paris, show a greater disagreement than those at Greenwich; and the observations of Mr. Piazzi at Palermo, a still greater than those of Mr. Lalande. This, I apprehend, must arise partly from the lesser elevation of the pole in those places, and partly from the fainter colours in the stellar spectra being more distinctly visible in the clear atmospheres of France and Italy than in England.

It should seem then, that in order to get a perfect knowledge of astronomical refraction, we ought to employ at least three different methods of investigation. 1st. By observations of the fixed stars during the night, when all the prismatic colours are visible. 2dly. By observations of the stars during the day, when none but the orange coloured rays are to be seen. And 3rdly, by observations of the sun with different coloured glasses. By these means we might hope to obtain such an accurate knowledge of atmospheric refraction as would enable us to form tables adapted to every possible circumstance.

* Vide Newton's Optics, Book I. Part II. Prop. III.

But I must not take up the time of the Society by any additional observations. It is in vain for me to pursue the subject any farther, in a situation so ill adapted to astronomical observations as Somerset Place; I shall therefore resign the task to those who are more favourably placed in this respect, and who possess instruments better calculated for an investigation which requires so much accuracy.

XXII. *Determination of the North Polar Distances and proper motion of thirty fixed Stars. By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read June 15, 1815.

When a standard catalogue of some of the principal fixed stars was laid before the Society in the year 1813, I ventured to state as my opinion, that the error of this catalogue depending on the mechanical construction of the instrument, did not probably exceed a quarter of a second.

This opinion has been confirmed by the observations of another year; the results of which I have now the honour of transmitting to the Society, as it appears that in those stars which I have continued to observe, I have not had occasion to alter the position of any one, above one-tenth of a second. For this reason I should hardly have thought it necessary to make any farther communication on the subject, had I not wished for an opportunity of adding some valuable deductions respecting the proper motions of these stars.

The comparison of my own catalogue with that of Dr. Bradley in the year 1756, is shown in one of the annexed tables, (Table II.) in which the proper motions are given in the last column.

I have also subjoined to my own observations the mean state of the barometer and thermometer, so that the correction may be easily made for any other table of refractions,

as well as that of Bradley, which I have employed in reducing the Greenwich observations.

Table III. contains, in addition to the standard catalogue, those stars which have been observed with equal care south of the equator, but from the uncertainty of refraction their positions cannot be so accurately ascertained as those of the former. In this table, the catalogue has been computed both by Bradley's and the French Tables of Refraction.

I.

Standard Catalogue of the North Polar Distances of thirty principal fixed Stars, reduced to the beginning of 1813.

	Stars.	No. of Observations in former Catalogue.	Result of one year's obser. N. P. D. January 1, 1813.	Total No. of Observations.	Result of two year's observ. N. P. D. January 1, 1813.	Mean height of barometer.	Mean height of thermometer. In.	Out.
			° ′ ″		° ′ ″	° ′	° ′	° ′
1	Polaris	167	1 41 21.7	294	1 41 21.66	29 79	49 0	48 0
2	β Urs. min.	90	15 4 48.9	120	15 4 48. 9	29 73	50 7	50 5
3	β Cephei	40	20 15 30.7	70	20 15 30. 6	29 77	49 2	45 5
4	α Urs. maj.	60	27 14 31.5	70	27 14 31. 5	29 81	56 0	55 0
5	α Cephei	40	28 12 12.5	70	28 12 12. 5			
6	α Cassiop.	40	34 29 22.7	70	34 29 22. 6			
7	γ Urs. maj.	48	35 15 55.3	60	35 15 55. 3			
8	γ Draconis	90	38 29 3.6	140	38 29 3. 6			
9	η Urs. maj.	80	39 44 57.9	100	39 44 57. 8			
10	α Persei	40	40 48 52.7	50	40 48 52. 6			
11	Capella	80	44 12 20.5	110	44 12 20. 4			
12	α Cygni	70	45 22 56.9	130	45 22 57. 1	29 77	46 8	43 4
13	α Lyræ	90	51 23 0.5	170	51 23 0. 5	29 82	51 0	49 5
14	Castor	30	57 42 46.7	40	57 42 46. 7	29 81	50 4	48 4
15	Pollux	40	61 31 56.3	50	61 31 56. 4	29 95	50 0	49 2
16	β Tauri	50	61 33 43.7	70	61 33 43. 7	29 92	46 3	44 4
17	α Androm.	35	61 56 29.6	35	61 56 29. 6	29 88	54 2	53 1
18	α Cor. Bor.	80	62 38 55.4	90	62 38 55. 4	29 80	56 8	56 2
19	α Arietis	50	67 25 36.5	80	67 25 36. 5	29 85	43 3	40 3
20	Arcturus	80	69 50 19.1	120	69 50 19. 0	29 82	55 0	54 9
21	Aldebaran	56	73 52 35.4	76	73 52 35. 3	29 91	50 7	50 3
22	β Leonis	20	74 22 57.3	20	74 22 57. 3	29 81	65 4	62 4
23	α Herculis	50	75 23 14.0	50	75 23 14. 0	29 88	57 4	55 9
24	α Pegasi	20	75 47 51.7	30	75 47 51. 6	29 69	48 3	44 4
25	Regulus	65	77 7 22.7	65	77 7 22. 7	29 89	54 7	54 5
26	α Ophiuchi	70	77 17 39.2	90	77 17 39. 1	29 86	56 4	54 3
27	α Aquilæ	80	81 36 58.8	140	81 36 58. 8	29 81	51 0	46 6
28	α Orionis	50	82 38 15.7	60	82 38 15. 7	29 96	53 5	53 3
29	α Serpentis	70	82 58 39.3	70	82 58 39. 3	29 86	58 3	57 3
30	Procyon	40	84 18 14.4	40	84 18 14. 4	29 96	55 4	55 4
31	Polaris SP.					29 79	59 1	52 2

II.

Observations made with the Mural Circle, compared with the observations of Dr. Bradley *in the year* 1756.

	Stars.	N. P. D. begin. 1756.	N. P. D. begin. 1814.	Variation in 58 years.	Precession in 58 years.	Difference.	Annual Proper Motion.
		° ′ ″	° ′ ″	′ ″	′ ″	′ ″	″
1	Polaris						
2	β Urs. min.	14 50 47.4	15 5 3.6	+ 14 16.2	14 10.4	0 5.8	+ 0.100
3	β Cephei	20 30 26.2	20 15 15.0	− 15 11.2	15 7.7	3.5	− 0.060
4	α Urs. maj.	26 56 18.5	27 14 50.7	+ 18 32.2	18 27.3	4.9	+ 0.084
5	α Cephei	28 26 27.9	28 11 57.6	− 14 30.3	14 27.3	3.0	− 0.052
6	α Cassiop.	34 48 14.5	34 29 2.9	− 19 11.6	19 11.7	0.1	+ 0.002
7	γ Urs. maj.	34 56 57.8	35 16 15.3	+ 19 17.5	19 17.2	0.3	+ 0.005
8	γ Draconis	38 28 22.0	38 29 4.3	+ 0 42.3	0 42.2	0.1	+ 0.002
9	η Urs. maj.	39 27 40.4	39 45 16.1	+ 17 35.7	17 33.9	1.8	+ 0.031
10	α Persei	41 1 47.2	40 48 39.2	− 13 8.0	13 7.6	0.4	− 0.007
11	Capella	44 16 51.5	44 12 15.9	− 4 35.6	4 56.4	20.8	+ 0.358
12	α Cygni	45 34 52.4	45 22 44.3	− 12 8.1	12 2.8	5.3	− 0.091
13	α Lyræ	51 25 47.1	51 22 57.4	− 2 49.7	2 29.9	19.8	− 0.341
14	Castor	57 36 10.7	57 42 53.8	+ 6 43.1	6 40.7	2.4	+ 0.041
15	Pollux	61 24 28.4	61 32 4.3	+ 7 35.9	7 34.1	1.8	+ 0.031
16	β Tauri	61 37 30.9	61 33 39.8	− 3 51.1	3 57.9	6.8	+ 0.117
17	α Androm.	62 15 27.3	61 56 9.6	− 19 17.7	19 20.4	2.7	+ 0.047
18	α Cor. Bor.	62 27 0.2	62 39 7.9	+ 12 7.7	12 4.3	3.4	+ 0.058
19	α Arietis	67 42 12.9	67 25 19.1	− 16 53.8	16 57.4	3.6	+ 0.062
20	Arcturus	69 32 13.6	69 50 38.1	+ 18 24.5	16 30.0	1 54.5	+ 1.972
21	Aldebaran	74 0 15.4	73 52 27.4	− 7 48.0	7 53.9	5.9	+ 0.102
22	β Leonis	74 3 55.6	74 23 17.3	+ 19 21.7	19 15.4	6.3	+ 0.109
23	α Herculis	75 18 46.1	75 23 18.5	+ 4 32.4	4 36.4	4.0	− 0.069
24	α Pegasi	76 6 10.6	75 47 32.3	− 18 38.3	18 32.9	5.4	− 0.093
25	Regulus	76 51 5.1	77 7 40.0	+ 16 35.0	16 37.1	2.1	− 0.036
26	α Ophiuchi	77 14 35.9	77 17 42.3	+ 3 6.8	2 56.9	9.5	+ 0.164
27	α Aquilæ	81 45 27.8	81 36 49.6	− 8 38.2	8 11.4	26.8	− 0.462
28	α Orionis	82 39 42.3	82 38 14.3	− 1 28.0	1 23.3	4.7	− 0.081
29	α Serpentis	82 47 24.7	82 58 51.0	+ 11 26.3	11 31.7	5.4	− 0.093
30	Procyon	84 10 10.3	84 18 21.9	+ 8 11.6	7 14.7	56.9	+ 0.981

The N. P. D. of Polaris determined by upwards of 200 observations of Dr. Bradley, by computations made under the direction of Dr. Maskelyne, a short time before his death, and

reduced to the beginning of the year - -	1749 -	2° 2′ 17″.25
By my observations for - - -	1813 -	1 41 21 .75
Variation in - - - -	64 years -	20 55 .50
Precession for - - - -	64 years -	20 51 .83
Difference - - - - - - - -		3 .67
Annual proper motion - - - - -		— 0.0 57

i. e. The annual precession, which is itself negative, must be increased by the above quantity.

III.

North Polar Distances of forty-four principal Stars for January 1, 1813.

	Stars.	With Bradley's Refraction.	With the French Refraction.	Annual variation.	Annual Proper Motion.
		° ′ ″	° ′ ″	″	″
1	Polaris	1 41 21.6	1 41 21.6	− 19.45	− 0.057
2	β Urs. min.	15 4 49.0	15 4 49.3	+ 14.70	+ 0.100
3	β Cephei	20 15 30.6	20 15 30.9	− 15.70	− 0.060
4	α Urs. maj.	27 14 31.5	27 14 31.9	+ 19.30	+ 0.084
5	α Cephei	28 12 12.5	28 12 12.7	− 14.96	− 0.052
6	α Cassiop.	34 29 22.7	34 29 23.1	− 19.80	+ 0.002
7	γ Urs. maj.	35 15 55.3	35 15 55.8	+ 20.00	+ 0.005
8	γ Draconis	38 29 3.7	38 29 4.2	+ 0.70	+ 0.002
9	η Urs. maj.	39 44 57.9	39 44 58.5	+ 18.20	+ 0.031
10	α Persei	40 48 52.6	40 48 53.2	− 13.50	− 0.007
11	Capella	44 12 20.5	44 12 21.1	− 4.57	+ 0.358
12	α Cygni	45 22 57.0	45 22 57.7	− 12.63	− 0.091
13	α Lyræ	51 23 0.5	51 23 1.2	− 3.00	− 0.341
14	Castor	57 42 46.7	57 42 47.5	+ 7.06	+ 0.041
15	Pollux	61 31 56.4	61 31 57.2	+ 8.00	+ 0.031
16	β Tauri	61 33 43.7	61 33 44.5	− 3.83	+ 0.117
17	α Andromedæ	61 56 29.6	61 56 30.3	− 19.99	+ 0.047
18	α Cor. Bor.	62 38 55.4	62 38 56.2	+ 12.49	+ 0.058
19	α Arietis	67 25 36.5	67 25 37.2	− 17.40	+ 0.062
20	Arcturus	69 50 19.0	69 50 19.8	+ 18.99	+ 1.972
21	Aldebaran	73 52 35.4	73 52 36.3	− 7.95	+ 0.102
22	β Leonis	74 22 57.3	74 22 58.5	+ 20.04	+ 0.109
23	α Herculis	75 23 14.0	75 23 15.1	+ 4.48	− 0.069
24	α Pegasi	75 47 51.6	75 47 52.8	− 19.43	− 0.093
25	γ	75 51 21.0	75 51 22.3	− 20.20	− 0.084
26	Regulus	77 7 22.7	77 7 23.9	+ 17.33	− 0.036
27	α Ophiuchi	77 17 39.1	77 17 40.3	+ 3.10	+ 0.164
28	γ Aquilæ	79 50 0.6	79 50 1.1	− 8.38	− 0.082
29	α	81 36 58.8	81 37 0.0	− 9.06	− 0.462
30	α Orionis	82 38 15.7	82 38 16.9	− 1.37	− 0.081
31	α Serpentis	82 58 39.3	82 58 40.6	+ 11.73	− 0.093
32	β Aquilæ	84 3 4.1	84 3 5.8	− 8.57	+ 0.391
33	Procyon	84 18 14.4	84 18 15.9	+ 7.55	+ 0.981
34	α Ceti	86 39 0.7	86 39 2.6	− 14.75	− 0.005
35	α Aquarii	91 13 21.6	91 13 23.8	− 17.37	− 0.106
36	α Hydræ	97 51 11.3	97 51 13.0	+ 15.19	− 0.066
37	Rigel	98 25 33.8	98 25 36.5	− 4.92	− 0.108
38	Spica Virginis	100 10 51.3	100 10 54.1	+ 18.95	+ 0.002
39	1 } α Capricorni	103 4 35.4	103 4 38.5	− 10.80	− 0.083
40	2 }	103 6 52.3	103 6 55.5	− 10.80	− 0.090
41	1 } α Libræ	105 12 38.7	105 12 42.0	+ 15.20	0.000
42	2 }	105 15 22.7	105 15 26.2	+ 15.20	+ 0.036
43	Sirius	106 28 0.7	106 28 4.2	+ 4.36	+ 1.158
44	Antares	116 0 16.6	116 0 22.2	+ 8.62	+ 0.012

XXIII. *An essay towards the calculus of functions. By* C. Babbage, *Esq. Communicated by W. H. Wollaston, M. D. Sec. R. S.*

Read June 15, 1815.

The term function has long been introduced into analysis with great advantage, for the purpose of designating the result of every operation that can be performed on quantity. This extent of signification has rendered it of essential use, but the various applications of which it admits, and the questions to which it gives rise, do not appear to have met with sufficient attention.

I propose in the following paper to present an outline of a new calculus, which naturally results from it. It comprehends questions of the greatest generality and difficulty, and will probably require the invention of new methods for its improvement.

Many of the calculations with which we are familiar, consist of two parts, a direct, and an inverse; thus, when we consider an exponent of a quantity: to raise any number to a given power, is the direct operation: to extract a given root of any number, is the inverse method. The differential calculus, which is a direct method, naturally gave rise to the integral, which is its inverse: the same remark is applicable to finite differences. In all these cases the inverse method is by far the most difficult, and it might perhaps be added, the most useful.

It is this inverse method with respect to functions, which I at present propose to consider.

If an unknown quantity as x, be given by means of an equation, it becomes a question how to determine its value; similarly if an unknown function as ψ, be given by means of any functional equation, it is required to assign its form. In the first case, it is quantity which is to be determined; in the second, it is the form assumed by quantity, that becomes the subject of investigation. In the one case, the various powers of the unknown quantity enter into the equation; in the other, the different orders of the function are concerned.

Before I proceed, it will be proper to explain the meaning of the order of a functional equation, and likewise to indicate the notation made use of; α, β, γ, &c. are known functional characteristics; ψ, χ, ψ, are unknown ones.

If in any function as ψx, instead of x, the original function be substituted, it becomes $\psi\psi x$ or $\psi^2 x$: this is called the second function of x. If the process be repeated, the result is $\psi^2 \psi x$ or $\psi^3 x$, the third function of x; and similarly $\psi^n x$, denotes the n^{th} function of x. Suppose

$$\psi x = a + x$$

then $$\psi^2 x = a + a + x = 2a + x$$

and generally $$\psi^n x = na + x$$

A functional equation is said to be of the first order, when it contains only the first function of the unknown quantity; as, for instance,

$$\psi \alpha x + x \psi x - x^n = 0$$

$$\left(\psi x + \psi \frac{1}{x}\right)^n - ax + x^2 = 0.$$

If the second function enter, the equation rises to the second order: thus,

$$\psi^2 x = x$$
$$\psi(x + \psi x) + (\psi x - x)^2 = o$$
$$\left(\psi^2 x + \psi \frac{1}{x}\right)^n = ax$$

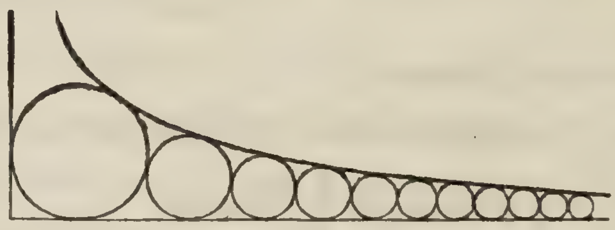
A function of two variables admits of two second functions: thus $\psi(x,y)$ becomes $\psi\ (\psi(x,y),y)$, and $\psi(x, \psi(x, y))$ or they might be thus expressed $\psi^{2,1}(x,y)$ and $\psi^{1,2}(x,y)$.

These express the second functions; the first taken relative to x, the other relative to y. But besides these two there is another, which arises from taking the second function simultaneously relative to x, and y; it is $\psi\left\{(\psi x,y), \psi(x,y)\right\}$. This ought not to be written $\psi^{2,2}(x,y)$ for it is not the second function first taken relative to x and then to y, nor is it the converse of this. In fact, the notation is defective; some method is wanting of indicating the order in which the successive substitutions are made. I shall for the present lay aside the consideration of functional equations, involving more than one variable.

Those of the first order have long been known, but the method in which I have treated them is, I believe, entirely new. Equations of the second and higher orders have never been even mentioned; it is these which present the most interesting speculations, and which are involved in the greatest difficulties. I shall first give some account of the enquiries which led me to this subject, and shall then treat of the various orders of functional equations.

Some few years since, while considering a problem mentioned by Pappus, relating to the inscription of a number

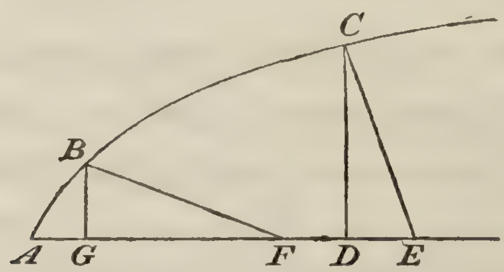
of circles in a semicircle, the following question occurred to me.



If in an hyperbola between its assymptotes a circle be inscribed touching both asymptotes and the curve, and if another circle be inscribed touching the first circle, the curve, and one asymptote, and if this be continued as represented in the figure, what ratio does the area of the circles bear to that of the figure; and conversely, if this ratio is given, what is the nature of the curve? I soon perceived the great difficulty of the subject, and that these and other problems similar to the latter of them, required the application of methods totally different from any with which I was then acquainted.

Hopeless of success, I laid aside the subject until about two years after, when the same difficulty recurred under another form. I had proposed to myself the following problem:

What must be the nature of the curve ABC, such that if



any point C be taken, and the ordinate CD, the normal CE, and subnormal DE be drawn, and if the triangle CDE be turned into such a position that CD may become the base and DE the perpendicular, if DE coincide with some new ordinate as GB, then the normal CE at the first point shall coincide with the normal BF at the second?

This latter question, of far less difficulty than the former, was readily solved, but I did not at first observe that they rested on similar principles; this, however, was pointed out by Mr. HERSCHEL, to whom I had mentioned the subject. Such was the origin of the following enquiries.

The question, in its most general point of view, is the solution of functional equations of all orders.

This, however, is a generality which I do not pretend to have attained. In the first part of this Paper the reader will find a new method of solving all functional equations of the first order; it depends on possessing their particular solutions. In the subsequent part, I have given various methods of solving functional equations of the second and higher orders: some of these possess considerable generality; and if we consider only those in which the n^{th} and inferior orders enter simply, such as

$$F\left\{x, \psi x, \psi^2 x, .. \psi^n x\right\} = 0$$

I have pointed out the means of obtaining their solutions.

The determination of functions from given conditions most probably took its rise from the integration of equations of partial differentials; and we accordingly find that the authors of this calculus were soon engaged in the new problem to which it gave birth. D'ALEMBERT was the first who occupied himself with this subject: he was soon followed by EULER and LAGRANGE; but it is to MONGE that we are indebted for the most general view of the subject. His enquiries were directed to the determination of two functions from given conditions; they are contained in the fifth volume of the *Melanges de Turin,* and in two Papers in the seventh volume of the *Memoires des Savans Etrangers,* 1773.

In the first of these he explains the solution of several functional equations by means of curves of double curvature, and by curve surfaces.

In the second Paper, the question is treated in a more analytical method, and he endeavours to reduce it to the solution of equations of differences. "Je me propose," observes MONGE, "de faire voir que la determination des fonctions arbitraires qui se trouvent dans l' integrale d'une equation aux différences partielles, depend en general, dans les cas que je n'ai pas encore traités, de l'integrale d'une ou de plusieurs équations aux differences finies, dans lesquelles le rapport de la variable principale à sa difference finie est donné soit qu'il soit variable soit qu'il soit constant."

In the same volume is a paper of LAPLACE on this subject, which he views in the same light, and endeavours to reduce functional equations of the first order to those of finite differences. This skilful analyst first solved the functional equation $F\left\{x, \phi x, \phi \alpha x,\right\} = o$. The method he made use of is peculiarly elegant; he converted it into an equation of finite differences in which the difference was constant. Still, however, it appeared by no means the most direct method to make use of such an expedient, nor was it even known that all equations of the first order admitted of its application. This latter objection was, however, removed by Mr. HERSCHEL, who in an excellent paper on functional equations, has extended the method made use of by LAPLACE to the solution of all equations of the first order. His solution is equally elegant and general; it leaves nothing to be regretted, but the narrow limits of our knowledge respecting the integration of equations of finite differences. From this and other causes, I am still

inclined to think that the solution of functional equations must be sought by methods peculiarly their own. There are some other researches on this difficult subject of which I am unable to give any account, from the impossibility of procuring the works in which they are contained; among these is the paper of ARBOGAST, which gained the prize of the Academy of Petersburg in the year 1790.

For the sake of convenience, I shall call any solution of a functional equation which contains one or more arbitrary functions, a general solution; but if the solution of such an equation only contains arbitrary constants, I shall call it a particular solution. With respect to the number of arbitrary functions that may enter into any solution, I shall make some observations at the conclusion of this paper.

PROBLEM I.

Required the general solution of the functional equation,

$$\psi x = \psi \alpha x$$

supposing we are acquainted with one particular solution.

Let the particular solution be $f x = f \alpha x$; then take $\psi = \phi f$, ϕ being an arbitrary function. It is evident that this value of ψ will satisfy the original equation, and that

$$\phi f x = \phi f \alpha x$$

is identical, because $f x = f \alpha x$.

Example, let the equation be

$$\psi (x) = \psi (-x)$$

and the particular solution be

$$f x = x^2$$

the general solution is

$$\psi x = \phi (x^2)$$

which evidently answers the conditions.

As I shall have frequent occasion to make use of symmetrical functions of two or more quantities, I shall for the sake of brevity denote this by putting a line over the functional characteristic; thus $\bar{\phi}\,(x,y)$ represents a symmetrical function of x and y, which it is well known possesses the following property,

$$\bar{\phi}\left\{x, y\right\} = \bar{\phi}\left\{y, x\right\}$$

As we are only considering functional equations of one variable, this will be sufficient for the present purpose; it might perhaps otherwise be more advisable to put the line over the quantities relative to which the function is symmetrical; thus $\phi\left\{x, y, \bar{z}, \bar{v}\right\}$ is symmetrical relative to z and v, but it is not so in respect to the other variables. This would possess the advantage of readily designating a function symmetrical relative to two quantities in one way, and likewise symmetrical with respect to two others, but in a different manner,* thus

$$\phi\left\{\bar{x}, \bar{y}, \overset{\text{I}}{\bar{v}}, \overset{\text{I}}{\bar{z}}\right\}$$

a particular case of this is

$$\frac{v + z + a\,x\,y}{v^3\,z^3 - a\,x^2\,y^2}$$

which is symmetrical in one sense relative to x and y, and in a different sense with respect to v and z; but these belong to other enquiries.

PROBLEM II.

Required a general solution of the equation $\psi\, x = \psi\, \alpha\, x$, having given a particular solution of $f\,x = f\,\alpha^2\,x$

* This is not a mere imaginary refinement; I have constantly had occasion to make use of functions of many variables which were symmetrical by pairs, when investigating the nature of functional equations of more than one variable.

Assume $\psi x = \bar{\phi}\{f x, f_{1} x\}$ then it becomes

$$\bar{\phi}\{f x, f_{1} x\} = \bar{\phi}\{f \alpha x, f_{1} \alpha x\}$$

this equation will be satisfied if we determine f and f_{1} so that the following equations may be fulfilled.

$$f \alpha x = f_{1} x \text{ and } f_{1} \alpha x = f x$$

from these result $f \alpha^{2} x = f_{1} \alpha x = f x$.

but we have by hypothesis a particular solution of $f \alpha^{2} x = f x$,

therefore the general solution of $\psi x = \psi \alpha x$ is

$$\psi x = \bar{\phi}\{f x, f \alpha x\}$$

If the function α should be of such a nature that $\alpha^{2} x = x$, or even if $\alpha^{2n} x = x$, $f x$ will then become x.

for example, suppose $\psi x = \psi \left(\frac{x}{ax-1}\right)$

then since $\alpha x = \frac{x}{ax-1}$, $\alpha^{2} x = \alpha \left(\frac{x}{ax-1}\right) = \dfrac{\frac{x}{ax-1}}{a\frac{x}{ax-1} - 1} = x$

and we have $\psi x = \bar{\phi}\left\{x, \frac{x}{ax-1}\right\}$

a particular case of this is when $a = o$, $\psi(x) = \psi(-x)$, its solution is

$$\psi x = \bar{\phi}\{x, -x\} = \phi(x^{2})$$

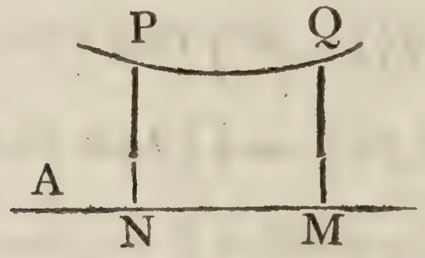
the same as in the last Problem; as another example take

$$\psi x = \psi \frac{1}{x}$$

then since $\alpha x = \frac{1}{x}$, $\alpha^{2} x = \dfrac{1}{\frac{1}{x}} = x$ and $\psi x = \bar{\phi}\left\{x, \frac{1}{x}\right\}$.

This affords a solution to the following question:

Required the nature of a curve, such that if any two abscissæ, whose rectangle is equal to a given square be taken, their corresponding ordinates may also be equal.



Let $y = \psi\, x$ be the equation of the curve, also AN $= x$, and if AN $\times$ AM $= a^2$, then the property of the curve is that

$$\text{PN} = \text{QM},$$

but PN $= y = \psi x$, and QM $= \psi$ (AM) $= \psi\left(\frac{a^2}{x}\right)$ consequently the equation from which ψ must be determined, is

$$\psi x = \psi \frac{a^2}{x}$$

its solutios in

$$y = \psi\, x = \bar{\varphi}\left\{x, \frac{a^2}{x^2}\right\},$$

if we make $y = x + \frac{a^2}{x}$, it becomes $yx - x^2 = a^2$, an equation to the hyperbola.

Problem III.

Required the general solution of $\psi\, x = \text{A}\, x \times \psi\, \alpha\, x$; having a particular solution, and also one of $\psi\, x = \psi\, \alpha^2\, x$.

Assume $\psi\, x = fx \times \bar{\varphi}\left\{\underset{1}{f}x, \underset{2}{f}x\right\}$

making this substitution in the original equation, it becomes

$fx \times \bar{\varphi}\left\{\underset{1}{f}x, \underset{2}{f}x\right\} = \text{A}\, x \times f\alpha\, x\, \bar{\varphi}\left\{\underset{1}{f}\alpha x, \underset{2}{f}\alpha x\right\}$ make $\underset{2}{f}\, \alpha\, x = \underset{1}{f}x$, and $\underset{1}{f}\alpha x = \underset{2}{f}x$, from this it results that $\underset{1}{f}x = \underset{1}{f}\alpha^2\, x$ of which we possess a particular solution, divide both sides by $\bar{\varphi}\left\{\underset{1}{f}x, \underset{1}{f}\alpha x\right\}$ then we have

$$fx = \text{A}x \times f\, \alpha x;$$

this is nothing more than the original equation of which, and of $\underset{1}{f}x = \underset{1}{f}\alpha^2\, x$, we have by hypothesis particular solutions:

therefore its general solution is,

$$\psi x = f x \times \bar{\varphi}\left\{ \underset{1}{f}x, \underset{1}{f}\alpha x \right\}$$

The same equation may be solved differently, if we are acquainted with particular solutions of the equations

$$\psi x = \psi \alpha x, \text{ and } \psi x = \mathrm{A} x \times \psi \alpha x.$$

Assume $\psi x = f x \times \varphi x$, it becomes

$$f x \times \varphi x = \mathrm{A} x \times f \alpha x \times \varphi \alpha x,$$

let $f x$ be the particular solution of the original equation, then

$$f x = \mathrm{A} x \times f \alpha x,$$

and by division

$$\varphi x = \varphi \alpha x,$$

but of this also we have given a particular solution, call it $\underset{1}{f}x$, therefore the general expression of ψx, is

$$\psi x = f x \times \varphi \underset{1}{f} x.$$

Let us take for example the equation

$$\psi x = \frac{1 - x^2}{1 - 2x} \psi \frac{x}{x - 1}$$

the particular value of ψx is

$$f x = 1 + x,$$

and the particular case of

$$\psi x = \psi \frac{x}{2x - 1} \quad \text{is} \quad \underset{1}{f} x = \frac{x^2}{x - 1},$$

from these considerations the general solution is

$$\psi x = (1 + x) \varphi \frac{x^2}{x - 1}$$

which will on trial be found to satisfy the equation.*

* Another particular solution of

$$\psi x = \psi \frac{x}{x - 1} \text{ is } \psi x = \cos. \log. (x - 1)$$

this gives for the general solution of

$$\psi x = \frac{1 - x^2}{1 - 2x} \psi \frac{x}{x - 1}$$

$$\psi x = (1 + x) \varphi (\cos. \log. (x - 1)).$$

As another example, take

$$\psi x = -\frac{1}{x}\psi\frac{1}{x}$$

the particular solutions are, that of the equation itself

$$f x = \frac{x-1}{x}$$

and that of $\psi x = \psi\frac{1}{x}$ is known to be $\bar{\varphi}\left(x, \frac{1}{x}\right)$, hence the general solution is

$$\psi x = \frac{x-1}{x}\bar{\varphi}\left(x, \frac{1}{x}\right)$$

which, as is readily seen, fulfills the condition.

PROBLEM IV.

Required the general solution of

$$\psi x = A x \times \psi \alpha x + B x,$$

having given a particular solution fx

Assume $\psi x = fx + \varphi x$, then $\psi \alpha x$ becomes $f \alpha x + \varphi \alpha x$, and the equation is

$$f x + \varphi x = A x \times f \alpha x + A x \times \varphi \alpha x + Bx,$$

subtracting from this the particular solution $fx = A x \times f \alpha x + B x$, which is given by hypothesis, there remains

$$\varphi x = A x \times \varphi \alpha x,$$

this equation may be solved by Prob. III, and we thence obtain the general solution of the given equation.

From this may readily be deduced the solution of the following equations:

$$\psi(\varepsilon^{y}) \pm (\varepsilon^{-y}) = Ay$$

suppose f is a particular solution, take $\psi y = fy + \chi y$, the equation becomes

$$f(\varepsilon^{y}) \pm f(\varepsilon^{-y}) + \chi(\varepsilon^{y}) \pm \chi(\varepsilon^{-y}) = Ay,$$

but we have from the particular solution

$$f(\varepsilon^{y}) \pm f(\varepsilon^{-y}) = Ay,$$

this subtracted from the former, leaves

$$\chi(\varepsilon^{y}) \pm \chi(\varepsilon^{-y}) = 0.$$

Let us first consider the upper sign, then a particular solution of

$$\chi(\varepsilon^{y}) = -\chi(\varepsilon^{-y}),$$

$$\text{is } \chi(y) = (\log. y)^{2n+1}.$$

If we take the lower sign, then a solution of

$$\chi(\varepsilon^{y}) = \chi(\varepsilon^{-y})$$

$$\text{is } \chi(y) = (\log. y)^{2n}.$$

From these considerations it appears, that the general solutions of the given equations are

$$\psi y = fy + \varphi\left\{(\log. y)^{2n+1}\right\}$$

$$\text{and } \psi y = fy + \varphi\left\{(\log. y)^{2n}\right\}$$

according as the upper or under sign is used.

The equation just solved was not constructed as an example to this particular rule, but is selected because it has actually occurred. It is used by Mr. HERSCHEL in the Philosophical Transactions, 1814, for the purpose of assigning the sums of several very curious series. He there observes, that when the upper sign is used ${}^{2n}\mathrm{L}(1+y)$, and when the lower takes place ${}^{2n+1}\mathrm{L}\,(1+y$, are particular solutions, these may therefore be generalized by the introduction of an arbitrary function

If $\psi\varepsilon^{y} + \psi\varepsilon^{-y} =$ a rational function of y containing only even powers,

or if $\psi\varepsilon^{y} - \psi\varepsilon^{-y} =$ a rational function of y containing only odd powers,

they admit of the following solutions, in the first case, let

$$\psi\varepsilon^{y} + \psi\varepsilon^{-y} = \underset{0}{a} + \underset{1}{a}y^{2} + \underset{2}{a}y^{4} + \&c. + \underset{n}{a}y^{2n}$$

assume $\psi y = \underset{0}{A} + \underset{1}{A}(\log. y)^2 + \&c. + \underset{n}{A}(\log. y)^{2n}$

$\psi \varepsilon^y$ becomes $\underset{0}{A} + \underset{1}{A} y^2 + \underset{2}{A} y^4 + \&c. + \underset{n}{A} y^{2n}$

$\psi \varepsilon^{-y}$ becomes $\underset{0}{A} + \underset{1}{A} y^2 + \underset{2}{A} y^4 + \&c. + \underset{n}{A} y^{2n}$

By comparing the co-efficients,

$$\underset{0}{A} = \tfrac{1}{2} \underset{0}{a} \quad \underset{1}{A} = \tfrac{1}{2} \underset{1}{a}, \quad \&c. \quad \underset{n}{A} = \tfrac{1}{2} \underset{n}{a}$$

and calling the right side of the equation $F(y^2)$, if

$$\psi \varepsilon^y + \psi \varepsilon^{-y} = F y^2,$$

a particular solution is

$$\psi y = \tfrac{1}{2} F \left\{ (\log. y)^2 \right\}$$

and similarly if

$\psi \varepsilon^y - \psi \varepsilon^{-y} = F(y) =$ a function containing only odd powers of y, one particular solution is

$$\psi y = \tfrac{1}{2} F \left\{ \log. y \right\}$$

and the general solutions may be readily deduced as above.

PROBLEM V.

To reduce the equation $\psi x + A x \times \psi \alpha x + \&c. + N x \times \psi \nu x + X = o$, to one in which the last term is wanting, by means of a particular solution,

Let $f x$ be the given solution, make $\psi x = f x + \phi x$, and substituting this value, the equation becomes

$$\left.\begin{array}{l} f x + A x \times f \alpha x + B x \times f \beta x + \&c. + N x \times f \nu x + X \\ + \phi x + A x \times \phi \alpha x + B x \times \phi \beta x + \&c. + N x \times \phi \nu x \end{array}\right\} = o$$

the upper line is by hypothesis equal to nothing, therefore the equation is reduced to this,

$$\phi x + A x \times \phi \alpha x + \&c. + N x \times \phi \nu x = o \qquad (1)$$

and if we can discover the general solution of this latter equation, that of the former may be readily found.

Supposing α, β, γ, &c. ν, were to become α, α^2, α^3, .. α^n, the equation (1) would be changed into

$$\phi x + Ax \times \phi\alpha x + Bx \times \phi\alpha^2 x + \&c. + Nx \times \phi\alpha^{n-1} x = o \quad (2)$$

after further if αx, is such a function of x, that $\alpha^n x = x$, if we are acquainted with one particular solution of (2), we may easily determine the general one thus:

Assume $\phi x = fx \times \overline{\chi} \left\{ x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x \right\}$

here we must observe, that since χ is symmetrical relative to all the quantities contained within the brackets, it is immaterial in what order they are placed, and from the condition that $\alpha^n x = x$, it follows that if we substitute $\alpha^k x$ for x, (k being successively equal to 1, 2, 3, 4, and $n-1$, we shall always have these values $x, \alpha x, \alpha^2 x \ldots \alpha^{n-1} x$ only differently arranged, from these considerations the equation (2) will become

$$o = (fx + Ax \times f\alpha x + Bx \times f\alpha^2 x + \&c. + Nx \times f\alpha^{n-1} x)$$
$$\overline{\chi} \left\{ x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x \right\};$$

this equation may be satisfied by making the factor which multiplies $\overline{\chi}$ equal to nothing, and this is always the case when f is a particular solution, hence

$$\phi x = fx \times \overline{\chi} \left\{ x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x \right\}.$$

Problem VI.

To find a function of x, such that if instead of x we successively substitute αx, βx, γx, &c. νx, the results shall all be equal to the original function; or in other words, to determine ϕx from the equations

$$\phi x = \phi \alpha x = \phi \beta x = \&c. = \phi \nu x$$

find f, so as to satisfy the equation

$$f x = f \alpha x \qquad (a)$$

take any particular value

Find $\underset{1}{f}$, so as to satisfy the equation

$$\underset{1}{f} f x = \underset{1}{f} f \alpha \beta x = \underset{1}{f} f \beta x \qquad (b)$$

for it is known from (a) that

$$f = f \alpha$$

take some particular value, and determine $\underset{2}{f}$ from the equation

$$\underset{2}{f}\underset{1}{f} f x = \underset{2}{f}\underset{1}{f} f \beta \gamma x = \underset{2}{f}\underset{1}{f} f \gamma x,$$

continue this as far as

$$\underset{n}{f}\underset{n-1}{f} \ldots f x = \underset{n}{f}\underset{n-1}{f} \ldots f \nu x. \qquad (n)$$

then φ being any arbitrary function

$$\varphi \left\{ \underset{n}{f}\underset{n-1}{f} \ldots \underset{1}{f} f x \right\}$$

will satisfy the conditions of the problem.

As an example, let it be required to find a function which shall not change, when for x we substitute x, $-x$, or $\frac{x}{\sqrt{x^2-1}}$ here $\alpha x = -x$, and $\beta x = \frac{x}{\sqrt{x^2-1}}$

(a) becomes $f(x) = f(-x)$,

hence $fx = \bar{\varphi} \left\{ x, -x \right\}$ as a particular case, take $f x = x^2$, then (b) becomes

$$\underset{1}{f} x^2 = \underset{1}{f} (\beta x)^2 = \underset{1}{f} \frac{x^2}{x^2-1}$$

whose solution is $\underset{1}{f} x = \bar{\varphi} \left\{ x, \frac{x}{x-1} \right\}$

take the case of $\underset{1}{f} x = \frac{x^2}{x-1}$

then we find

$$\phi \underset{1}{f} f x = \phi \left\{ \frac{x^4}{x^2 - 1} \right\}$$

which fulfils the given conditions.

In the same manner it may be found, that the function

$$\phi \left\{ \frac{1 - ax^2 + x^4}{1 + ax^2 + x^4} \right\}$$

will remain the same, whether the variable is x, $-x$, or $\frac{1}{x}$.

PROBLEM VII.

Given any two series of functions αx, βx, &c. νx and $\underset{1}{\alpha} x$, $\underset{1}{\beta} x$, &c. $\underset{1}{\nu} x$, required the form of ψ, so that the following equations may be fulfilled,

$$\psi \alpha x = \psi \underset{1}{\alpha} x$$

$$\psi \beta x = \psi \underset{1}{\beta} x$$

$$\text{\&c.} \qquad \text{\&c.}$$

$$\psi \nu x = \psi \underset{1}{\nu} x.$$

Determine f from the equation

$$f \alpha x = f \underset{1}{\alpha} x \qquad (a)$$

take some particular case, and determine $\underset{1}{f}$ from the equation

$$\underset{1}{f} f \alpha \beta x = \underset{1}{f} f \underset{1}{\alpha} \underset{1}{\beta} x = \underset{1}{f} f \alpha \underset{1}{\beta} x \qquad (b)$$

take a particular case and find $\underset{2}{f}$ from the equation

$$\underset{2}{f} \underset{1}{f} f \alpha \beta \gamma x = \underset{2}{f} \underset{1}{f} f \underset{1}{\alpha} \underset{1}{\beta} \gamma x = \underset{2}{f} \underset{1}{f} f \alpha \beta \underset{1}{\gamma} x \qquad (c)$$

and continue this to

$$\underset{n}{f} \underset{n-1}{f} \ldots f \alpha \beta \gamma \ldots \nu x = \underset{n}{f} \underset{n-1}{f} \ldots f \alpha \beta \gamma \ldots \underset{1}{\nu} x \qquad (n)$$

Then will

$$\psi x = \phi \underset{n}{f} \underset{n-1}{f} \ldots f \alpha \beta \gamma \ldots \nu x$$

satisfy the conditions.

We now possess the means of solving a much more general problem than we have yet attempted, it is the general solution of any functional equation of the first order.

PROBLEM VIII.

Required the general solution of the equation

$$F\left\{x, \psi x, \psi \alpha x, \ldots \psi \nu x\right\}$$

having given a particular one, containing one or more arbitrary constants.

Find from Problem VI. an arbitrary function, such that it shall remain the same, when αx, βx, &c. νx, are substituted for x, call this ϕx, and let the particular solution be

$$\psi x = f\left\{x, a, b, c, \text{\&c.}\right\}$$

As these arbitrary constants are supposed not to be contained in the given equation $F\left\{x, \psi x, \psi \alpha x, \ldots \psi \nu x\right\} = o$, when the particular solution is substituted, they must destroy each other, whatever be their value.

If therefore, instead of a, b, c, &c. we put ϕx, $\underset{1}{\phi} x$, $\underset{2}{\phi} x$, &c. (which are arbitrary functions fulfilling this condition, viz. that they remain identical when αx, βx, &c. are pu for x:) it is evident that these arbitrary functions must also destroy each other, therefore

$$\psi x = f\left\{x, \phi x, \underset{1}{\phi} x, \underset{2}{\phi} x, \text{\&c.}\right\}$$

is a general solution containing as many arbitrary functions as the particular solution did constant quantities

Examples, let

$$x \psi x = \psi \frac{1}{x}$$

a particular solution is $\psi x = a \frac{x+1}{x}$

and $\bar{\varphi} \left\{ x, \frac{1}{x} \right\}$ does not change when x becomes $\frac{1}{x}$, therefore the general solution is

$$\psi x = \frac{x+1}{x} \bar{\varphi} \left(x, \frac{1}{x} \right)$$

take $$\psi x - \psi \frac{1}{x} = a\,(1 - x^2)$$

a particular case of this equation is

$$\psi x = a + b x$$

therefore, the general solution is

$$\psi x = a + \bar{\varphi} \left\{ x, \frac{1}{x} \right\}.$$

Let $\psi\,(x) \times \psi\,(-x) = \frac{1+x}{1-x} \left(\frac{1}{x} \psi \frac{1}{x} \right)^2$

as a particular solution take

$$\psi x = \frac{1-x}{ax}$$

and $\varphi \left\{ \frac{1 - ax^2 + x^4}{1 + ax^2 + x^4} \right\}$ is a functional equation which does not change when x becomes $-x$ or $\frac{1}{x}$, therefore the general solution is

$$\psi x = \frac{1 - x}{x \varphi \left\{ \frac{1 - a x^2 + x^4}{1 + a x^2 + x^4} \right\}}$$

φ being arbitrary.

Given the equation

$$2 \psi \left(\frac{x}{\sqrt{x^2 - 1}} \right) = \psi x + \psi\,(-x) + \frac{\psi x - \psi\,(-x)}{\sqrt{x^2 - 1}}$$

a particular solution is

$$\psi x = a + b x$$

hence the general solution will be found to be

$$\psi x = \varphi \left(\frac{x^4}{x^2 - 1} \right) + x \underset{1}{\varphi} \left(\frac{x^4}{x^2 - 1} \right)$$

On the number of arbitrary functions introduced into the complete solution of a functional equation.

When from a functional equation of the first order, we determine the form of the unknown function, one or more constant quantities are generally introduced; these as I have shown in a preceding Problem, may be changed into arbitrary functions of the unknown quantity which fulfil certain prescribed conditions.[1]

A question naturally arises as to the number of these arbitrary functions, and how many any given equation admits of in its most general solution.

The train of reasoning usually made use of to prove, that a differential equation of the n^{th} order, requires in its complete integral n, arbitrary constants may be pursued on the present occasion, though from several reasons, it would perhaps be desirable to have a proof resting on a different principle; as I have not been successful in discovering any other, I shall give the only one I am at present possessed of.

Let $$\psi\, x = F\left\{ x, a_1, a_2, \ldots a_n \right\}$$

for x, put any number of known functions, as αx, βx, .. νx, the results will be

$$\psi\, x = F\left\{ x, a_1, a_2, \ldots a_n \right\} \qquad (0)$$

$$\psi\, \alpha x = F\left\{ \alpha x, a_1, a_2, \ldots a_n \right\} \qquad (1)$$

$$\psi\, \beta x = F\left\{ \beta x, a_1, a_2, \ldots a_n \right\} \qquad (2)$$

&c. &c.

$$\psi\, \nu x = F\left\{ \nu x, a_1, a_2, \ldots a_n \right\} \qquad (n)$$

From these $n+1$ equations we may eliminate the n arbitrary constants, and the resulting equation will be of the form

$$o = \underset{1}{F}\left\{x, \psi x, \psi \alpha x, \,.\,.\, \psi \nu x\right\} \qquad (A)$$

In arriving at this equation, we have eliminated n arbitrary constants, and therefore it might possibly be inferred that the general solution of (A) is

$$\psi\, x = F\left\{x, \underset{1}{a}, \underset{2}{a}, \,.\,.\,.\, \underset{n}{a}\right\}.$$

But this is too hasty a conclusion, for it is evident, that we should equally have arrived at equation (A), if each of the constant quantities in (o) had been changed into a function of x so constituted that it should not alter by the substitution of αx, βx, &c. νx.

It would now appear, that putting such values for the constant quantities, the result would be the general solution of (A).

This reasoning is certainly plausible, and such a solution is undoubtedly a very general one; still, however, there are reasons which incline me to believe, that other solutions exist of a yet more general nature.

On functional equations of the second and higher orders.

When we consider functional equations of an order superior to the first, new difficulties present themselves; the artifices which were used with success in the preceding part of this paper, are no longer of any avail.

Those which we have now to consider seem to possess an entirely distinct character.

Problem IX.

Required the solution of the equation

$$\psi^2 x = x \qquad (a)$$

Subtract ψx from both sides, then we have

$$\psi^2 x - \psi x = -\psi x + x = -(\psi x - x)$$

consequently,

$$\Delta \psi x = -\Delta x \qquad (b)$$

again multiply (a) by ψx, then we have

$$\psi^2 x \times \psi x = \psi x \times x$$

From this we learn, that (b) must be integrated on the hypothesis of $x \psi x$ being constant, hence

$$\psi x = -x + c = -x + f(x \psi x)$$

f being an arbitrary function

ψ is determined from the equation

$$\psi x + x - f(x \psi x) = o.$$

For f we may put $\underset{1}{f}^{-1} f$, and it becomes

$$\underset{1}{f}(x + \psi x) - f(x \psi x) = o \qquad (c)$$

A friend to whose valuable remarks on this subject, I am much indebted, has communicated to me the following method of obtaining the same solution. Assume any symmetrical function of x and v.

$$\bar{\phi}(x, v) = o$$

then from the nature of the equation we have the two following equations $\quad x = \psi v$ and $v = \psi x$

consequently

$$x = \psi v = \psi \psi x = \psi^2 x,$$

hence ψx may be found from the equation

$$\bar{\phi}\left\{x, \psi x\right\} = o$$

This at first sight appears different from (c), but it is not so

in reality, for it may easily be shown, that from the sum and product of two quantities any symmetrical function may be composed.

As particular cases of the equation

$$\psi^2 x = x$$

we may notice

$$\psi x = a - x, \quad \frac{x}{ax-1}, \quad \frac{b-x}{1-ax}, \text{ and } \frac{ax+\sqrt{4+a^2-4x^2}}{2}$$

PROBLEM X.

Required the solution of

$$\psi^2 x = x$$

another solution may be found from the following principle.

Assume $\quad \psi x = \overset{-1}{\varphi} f \varphi x$

hence $\quad \psi^2 x = \overset{-1}{\varphi} f \varphi \overset{-1}{\varphi} f \varphi x = \overset{-1}{\varphi} f^2 \varphi x$

or $\quad \overset{-1}{\varphi} f^2 \varphi x = x.$

Now this equation can be satisfied if we are acquainted with a particular solution of $f^2 x = x$, this may be found from the last problem, and since $f^2 x = x$, the equation becomes

$$\overset{-1}{\varphi} \varphi x = x$$

which is identical, consequently

$$\psi x = \overset{-1}{\varphi} f \varphi x$$

some particular cases are

$$\psi x = \overset{-1}{\varphi} (a - \varphi x,), \quad \overset{-1}{\varphi} \left(\frac{\varphi x}{a \varphi x - 1} \right), \quad \overset{-1}{\varphi} \left(\frac{a}{\varphi x} \right) \text{ \&c.}$$

from each of these by assigning particular values to φ, new values of f may be determined, and these in their turn will furnish new forms of the function ψx.

Some time ago I received from the gentleman already alluded to, the following solution of the equation

$$\psi^2 x = x$$

$$\psi x = \varphi(-\varphi x)$$

and likewise the solution $\overset{-1}{\varphi}\left((-1)^{\frac{1}{n}}\varphi x\right)$ of the equation $\psi^n x = x$; this first led me to the substitution of $\overset{-1}{\varphi} f \varphi x$, which is of such essential use in these enquiries.

PROBLEM XI.

Given the equation.

$$\psi^n x = x.$$

Assume as before $\psi x = \overset{-1}{\varphi} f \varphi x$,

then
$$\psi^2 x = \overset{-1}{\varphi} f \varphi \overset{-1}{\varphi} f \varphi x = \overset{-1}{\varphi} f^2 \varphi x$$
$$\psi^3 x = \overset{-1}{\varphi} f^2 \varphi \overset{-1}{\varphi} f \varphi x = \overset{-1}{\varphi} f^3 \varphi x,$$

and generally $\psi^n x = \overset{-1}{\varphi} f^n \varphi x$,

hence our equation becomes

$$\overset{-1}{\varphi} f^n \varphi x = x. \qquad (a)$$

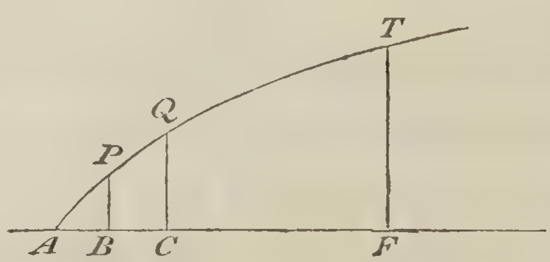
Suppose we have one particular solution of the equation, substitute this instead of f, and the equation (a) becomes identical, whence

$$\psi x = \overset{-1}{\varphi} f \varphi x$$

and from this other values of f may be determined, and so on ad infinitum.

The equation we have just considered, affords a ready solution of the following Problem.

Required the nature of a curve, such that taking any point B in the abscissa, and drawing the ordinate BP



if we make AC another abscissa equal to BP the preceding ordinate, and if we continue this n times, then the n^{th} ordinate may be equal to the first abscissa.

If AB $= x$ and the equation of the curve be $y = \psi x$.

PB $= y = \psi x$ and AC $=$ PB $= \psi x$,

and QC $= \psi \psi x = \psi^2 x$, and generally the n^{th} ordinate TF is equal to $\psi^n x$, hence

$$\psi^n x = x$$

which is the equation whose solution has been just found.

Problem XII.

Given the equation

$$\psi^2 x = \alpha x$$

required the form of ψ.

Assume $\qquad \psi x = \overset{-1}{\phi} f \phi x$

then $\qquad \psi^2 x = \overset{-1}{\phi} f^2 \phi x$

and $\qquad \overset{-1}{\phi} f^2 \phi x = \alpha x$

take the function ϕ on both sides, then this becomes

$$f^2 \phi x = \phi \alpha x.$$

This is a functional equation of the first order relative to ϕ, and may be solved either by the methods exhibited in the first part of this paper, or by the very elegant one of Laplace, f is a perfectly arbitrary function, except that neither fx nor $f^{\circ} x$ must be equal to x: from not attending to this circumstance, I was at first led into several errors; the reason of these two restrictions is, that in the first case we at once determine ψx to be equal to x, and in the second, we in fact make $\alpha x = x$, neither of which are necessarily true.

Problem XIII.

Given the equation.

$$\psi^n x = \alpha x$$

This admits of a solution similar to the last, by assuming ψx

equal to $\overset{-1}{\phi} f \phi x$, we find

$$\overset{-1}{\phi} f^n \phi x = \alpha x$$

and by taking ϕ on both sides, it becomes

$$f^n \phi x = \phi \alpha x$$

give f some particular value and determine ϕ as in the last problem.

Problem XIV.

Given the equation

$$\psi \alpha \psi \beta x = \gamma x$$

Assume $$\psi x = \overset{-1}{\phi} f \phi \underset{1}{f} x$$

hence $$\psi \alpha \psi \beta x = \overset{-1}{\phi} f \phi \underset{1}{f} \alpha \overset{-1}{\phi} f \phi \underset{1}{f} \beta x$$

make $\underset{1}{f} = \overset{-1}{\alpha}$, then the equation becomes

$$\overset{-1}{\phi} f \phi \overset{-1}{\phi} f \phi \overset{-1}{\alpha} \beta x = \overset{-1}{\phi} f^2 \phi \overset{-1}{\alpha} \beta x = \gamma x$$

and by taking ϕ on both sides

$$f^2 \phi \overset{-1}{\alpha} \beta x = \phi \gamma x$$

this is an equation of the first order relative to ϕ.

The equation $\psi \alpha \psi \alpha \ldots (n) \psi \beta y = \gamma y$ might be solved in the same manner, but they are both reducible by a simple transformation to the form

$$\psi^n y = \mathrm{F} y$$

Problem XIV.

Another method of solving the equation

$$\psi^2 x = x$$

Assume $\psi x = \overset{-1}{\phi} f \phi x$, then $\psi^2 x = \overset{-1}{\phi} f^2 \phi x$, and we have

$$\overset{-1}{\phi} f^2 \phi x = x.$$

Let $y = \phi x$, then $x = \overset{-1}{\phi} y$ and the equation becomes

$$\overset{-1}{\phi} f^2 y = \overset{-1}{\phi} y$$

an equation of the first order from which y may be found or thus assume

$$\overset{-1}{\phi}\, y = \overset{-1}{\chi} \left\{ y, f^2 y \right\}$$

and make $f^4 y = y$,

this method is much more extensive in its application than any of those before it.

Problem XVI.

Required the solution of

$$\psi^n x = x$$

the same method applies equally in this case,

Assume $\qquad \psi x = \overset{-1}{\phi} f \phi x$

then $\qquad \overset{-1}{\phi} f^n \phi x = x$

for x put $\overset{-1}{\phi}\, x$ it becomes

$$\overset{-1}{\phi} f^n x = \overset{-1}{\phi}\, x.$$

Take $\overset{-1}{\phi}$ an arbitrary symmetrical function of

$$x, f^n x, f^{2n} x, \text{ and } f^{kn} x,$$

then $\qquad \overset{-1}{\phi}\, x = \chi \left\{ x, f^n x, f^{2n} x, \ldots f^{kn} x \right\}$

and determine f such that

$$f^{(k+1)n} x = x$$

a particular solution is sufficient, and it is evident, this value of $\overset{-1}{\phi}$ will satisfy the equation.

Problem XVII.

Given the equation

$$\psi \alpha (y, \psi y) = y$$

required the form of y.

Assume $\psi y = \overset{-1}{\phi} f \phi y$, then it becomes

$$\overset{-1}{\phi} f \phi \alpha (y, \overset{-1}{\phi} f \phi y) = y,$$

take successively on each side the functions ϕ, f^{-1}, and $\overset{-1}{\phi}$, the equation becomes

$$\alpha\left(y, \overset{-1}{\phi} f \phi y\right) = \overset{-1}{\phi} f^{-1} \phi y$$

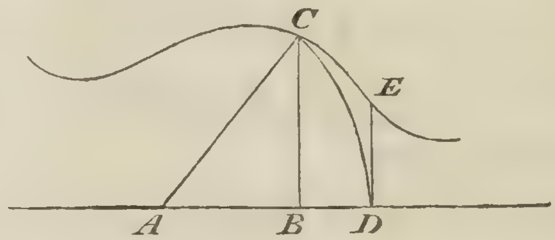
for y put $\overset{-1}{\phi}\, y$, then

$$\alpha\left(\overset{-1}{\phi} y, \overset{-1}{\phi} f y\right) = \overset{-1}{\phi} f^{-1} y,$$

this is a functional equation of the first order relative to $\overset{-1}{\phi}$, give f any determinate value and solve the equation.

From hence we may deduce the solution of the following question.

Required the nature of a curve



such that taking any abscissa AB and drawing the ordinate CB, if with centre A and radius AC, we describe a circle cutting the abscissa in D, the ordinate ED may be equal tó the first abscissa AB.

Let $y = \psi x = CB$

$$AD = CA = \sqrt{x^2 + (\psi x)^2}$$

and $ED = \psi (AD)$, hence

$$x = \psi \left(\sqrt{x^2 + (\psi x)^2} \right)$$

which is a particular case of the preceding problem.

Problem XVIII.

To reduce the equation

$$F\left(x, \psi x, \psi^2 \alpha x, \ldots \psi^{n} \nu\, x\right) = o$$

to one of the form of

$$\mathrm{F}\left(x, \psi x, \psi^{2} x, \ldots \psi^{n} x\right)=o.$$

Assume $\psi x=\overset{-1}{\mathrm{A}} \phi \mathrm{A} x,$ the equation by this substitution becomes

$$\mathrm{F}\left\{x, \overset{-1}{\mathrm{A}} \phi \mathrm{A} x, \overset{-1}{\mathrm{A}} \phi^{2} \mathrm{A} \alpha x, \ldots \overset{-1}{\mathrm{A}} \phi^{n} \mathrm{A} \nu x\right\}=o$$

find by Problem VI. such a value $\mathrm{A}x$ that it shall not change by the substitution of $\alpha x, \beta x, \gamma x,$ &c. νx, put for $\mathrm{A}x$ the quantity y, and the equation becomes

$$\mathrm{F}\left\{\overset{-1}{\mathrm{A}} y, \overset{-1}{\mathrm{A}} \phi y, \overset{-1}{\mathrm{A}} \phi^{2} y, \ldots \overset{-1}{\mathrm{A}} \phi^{n} y\right\}=o$$

which is an equation of the required form.

Problem XIX.

Required the solution of the equation

$$\mathrm{F}\left\{x, \psi x, \psi^{2} x, \ldots \psi^{n} x\right\}=o$$

Assume $\psi x=\overset{-1}{\phi} f \phi x$, then $\psi^{n} x=\overset{-1}{\phi} f^{n} \phi x$, and the equation becomes

$$\mathrm{F}\left\{x, \overset{-1}{\phi} f \phi x, \overset{-1}{\phi} f^{2} \phi x, \ldots \overset{-1}{\phi} f^{n} \phi x\right\}=o$$

for x substitute $\overset{-1}{\phi} x$, then

$$\mathrm{F}\left\{\overset{-1}{\phi} x, \overset{-1}{\phi} f x, \overset{-1}{\phi} f^{2} x, \ldots \phi^{\prime} f^{2} x\right\}=o \qquad (\bar{a})$$

which is an equation of the first order relative to $\overset{-1}{\phi}$ and may be solved by the methods in the beginning of this Paper, or, by means of the method given by Mr. Herschel, to which we have already alluded.

With respect to the function f it is arbitrary, there are however, some observations respecting it, which require notice; as without an attention to them we might fall into error. In the first place, it is evident, that we must not

make fx equal to x, for in this ease we at once determine ψx to be equal to x, which is not always true.

The same observation may be made with respect to making $f^2 x = x$, for in this case $\psi^2 x = x$, $\psi^3 x = \psi x$, $\psi^4 x = x$, $\psi^5 x = \psi x$, and we in fact by assuming this value for f determine ψ from the equation

$$F \left\{ x, \psi x, x, \psi x, x, \&c. \right\} = o$$

The same objection does not hold when we make $f^3 x = x$, though this considerably limits the generality of the solution; apparently the most eligible mode of determining f is from the equation $f^{n+1} x = x$, for in this case supposing we are acquainted with a particular solution of (a) containing any number of arbitrary constants, such as

$$\overset{-1}{\varphi} x = A \left\{ x, a, b, c, \&c. \right\}$$

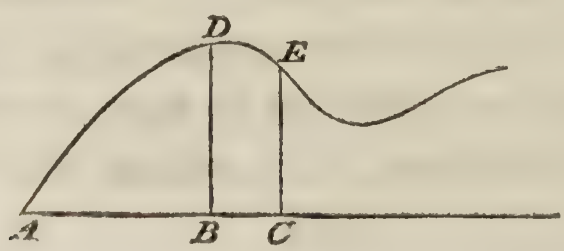
we may substitute for each of these constants an arbitrary function, such as

$$\bar{\chi} \left\{ x, fx, f^2 x, \ldots f^n x \right\}$$

for it is evident this function does not change when we substitute $f^m x$ for x.

But this form of f is not always correct, it may be inconsistent with the original equation. In fact, the only value we can assign to f which shall not in some measure limit the generality of the Problem, is to suppose a particular solution of the given equation.

As an example of this method take the following problem.



Required the nature of the curve ADE such that taking any abscissa AB and corresponding ordinate DB: if the abscissa AC be taken, equal to DB and the ordinate EC be drawn, then the rectangle under the two ordinates shall be equal to the square of the first abscissa let the equation of the curve be $y = \psi x$ then $\mathrm{AB} = x$ $\quad \mathrm{DB} = y = \psi x \quad \mathrm{AC} = \mathrm{DB}$,
and $\quad \mathrm{EC} = \psi\,(\mathrm{AC}) = \psi\,(\mathrm{DB}) = \psi y = \psi\psi x$
the given condition is therefore

$$\psi^2 x \times \psi x = x^2$$

making the usual substitution of $\psi x = \overset{-1}{\phi} f \phi x$ it becomes

$$\overset{-1}{\phi} f^2 \phi x \times \overset{-1}{\phi} f \phi x = x^2$$

putting $\overset{-1}{\phi} x$ for x we have

$$\overset{-1}{\phi} f^2 x \times \overset{-1}{\phi} f x = (\overset{-1}{\phi} x)^2$$

Assume $\quad \overset{-1}{\phi} x = \bar{\chi}(x, fx, f^2x)$

the equation then becomes

$$\bar{\chi}\left\{f^2x, f^3x, f^4x\right\} \times \bar{\chi}\left\{fx, fx^2, fx^3\right\} = \left[\bar{\chi}\left\{x, fx, f^2x\right\}\right]^2$$

If now f be determined from the equation

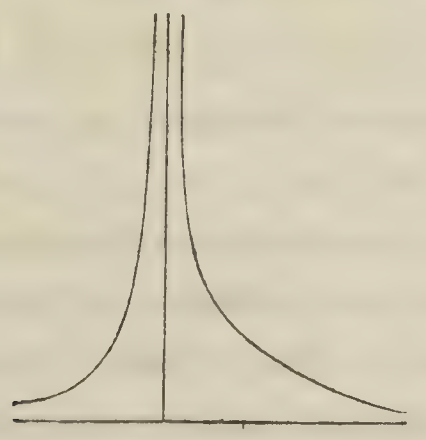
$$f^3 x = x$$

we have $f^4 x = fx$, and the equation becomes identical, hence $\overset{-1}{\phi}$ and f being found, we may determine ψ.

One of the simplest curves which satisfies the equation is

$$y x^2 = a^3$$

a line of the third order, the 69th in NEWTON's arrangement, its form is



PROBLEM XX.

Required the general solution of the equation

$$F\left\{x, \psi x, \psi^2 \alpha x, \psi^3 \beta x, \ldots \psi^n \nu x\right\} = o$$

Reduce this by Problem XVIII. to the form of

$$F\left\{x, \psi x, \psi^2 x, \ldots \psi^n x\right\} = o$$

in which shape it may be solved by the preceding problem.

PROBLEM XXI.

Given the equation

$$F\left\{x, \psi x, \psi\alpha\,(x, \psi\beta x)\right\} = o$$

required its solution.

Assume $\qquad \alpha\,(x, \psi x) = \overset{-1}{\phi} f \phi x \qquad (a)$

hence $\qquad \psi x = \alpha^{1,-1}\,(x, \overset{-1}{\phi} f \phi x) \qquad (b)$

In the left side of this equation, put for x the left side of (a) and in the right side, put instead of x the right side of (a) the result is

$$\psi\alpha\,(x, \psi x) = \alpha^{1,-1}\,(\overset{-1}{\phi} f \phi x, \overset{-1}{\phi} f^2 \phi x)$$

substituting this in the original equation, the result is

$$F\left\{x, \alpha^{1,-1}\,(x, \overset{-1}{\phi} f \phi x), \alpha^{1,-1}(\overset{-1}{\phi} f \phi x, \overset{-1}{\phi} f^2 \phi x,)\right\} = o$$

which by putting $\overset{-1}{\phi} x$ for x becomes

$$F\left\{\overset{-1}{\phi} x, \alpha^{1,-1}\,(\overset{-1}{\phi} x, \overset{-1}{\phi} f x), \alpha^{1,-1,}(\overset{-1}{\phi} f x, \overset{-1}{\phi} f^2 x)\right\} = o$$

this being an equation of the first order relative to $\overset{-1}{\phi}$ may be solved as above.

With respect to the number of arbitrary functions which enter into the complete solution of functional equations of higher orders than the first, I have little at present to offer; the difficulty of the subject, and the wide extent of the enquiries

to which it would lead, induce me to postpone it until I have more time for the consideration. The following remarks may suffice for the present to point out some of its difficulties and the mode of enquiry.

If
$$\psi x = f_1 \left\{ x, a, b, \text{\&c.} \right\} \quad (1)$$
$$\left.\begin{array}{l} \psi^2 x = f_2 \left\{ x, a, b, \text{\&c.} \right. \quad (2) \\ \psi^3 x = f_3 \left\{ x, a, b, \text{\&c.} \right. \quad (3) \end{array}\right\}$$
$$\text{\&c.} \qquad \text{\&c.}$$
$$\psi^n x = f_n \left\{ x, a, b, \text{\&c.} \right\} \quad (n)$$

From this by eliminating $n-1$ of the arbitrary constants a, b, &c. we have an equation of the form
$$F \left\{ x, \psi x, \psi^2 x, \ldots \psi^n x \right\} = 0 \qquad (a)$$
and it might possibly be concluded that equation (1) containing $n-1$ arbitrary constants is the general solution of this last equation: but this is by no means the case. In the first place between the two equations (1) and (2), more than one arbitrary constant may be eliminated, thus let
$$\psi x = \frac{a-x}{1-bx}$$
from which we find
$$\psi^2 x = x$$
the two quantities a and b have been eliminated, and it is possible to select a value of ψx, between which and $\psi^2 x$ an infinite number of arbitrary constants could be eliminated.

But waving this objection let us consider the case of (a) which is deduced from the elimination of $n-1$ arbitrary functions.

We have seen in Problem VI. that a function of the first

order may satisfy any number of conditions (which are not contradictory) simultaneously; and there appears no reason for denying this property to those of higher orders.

If now we consider the symmetrical function

$$\overline{\chi} \left\{ x, \psi x, \psi^2 x, \ldots \psi^v x \right\}$$

and if $\psi^{v+1} x = x$. It is evident this function will not change by the substitution of ψx, $\psi^2 x$, &c. or $\psi^n x$ and consequently that a different function similarly constituted may be substituted for each of the arbitrary quantities a, b, c, &c. in (1) which is the solution of the equation

$$F \left\{ x, \psi x, \psi^2 x, \ldots \psi^n x \right\} = 0 \qquad (a)$$

The number denoted by v is arbitrary (it may, however, become determined from some particular circumstances of the equation (a).

Thus we have introduced into an equation of the n^{th} order, an unlimited number of arbitrary functions, each of which contains the function whose determination was sought with all its different orders to an undefined extent.

If we take the particular case of

$$\psi x = a - x$$

$$\psi^2 x = x$$

v must be unity, and a general solution is

$$\psi x = \overline{\chi} \left\{ x, \psi x \right\} - x$$

taking another solution

$$\psi x = \frac{a - x}{1 - bx}, \qquad \psi^2 x = x$$

and

$$\psi x = \frac{\overline{\chi}(x, \psi x) - x}{1 - x\,\overline{\chi}(x, \psi x)}$$

If $\psi^n x = x$ $\qquad v = n - 1$, and supposing $\psi x = f(x, a, b, \text{\&c.})$ any particular solution we have for the general one

$$\psi x = f\left\{x, \bar{\chi}\,(x, \psi x, \ldots \psi^{n-1} x), \underset{1}{\bar{\chi}}\,(x, \psi x, \ldots \psi^{n-1} x)\ \&c.\right\}$$

from which equation of the $\overline{n-1}^{\text{th}}$ degree ψ must be found.

When we apply these considerations to functional equations of many variables, other and even greater difficulties present themselves; the first step in that direction must be an improvement in the notation.

Since the above was written, I have bestowed some attention on functional equations involving two or more variables, and I have met with considerable success: I am in possession of methods which give the general solution of equations of all orders, and even of those which contain symmetrical functions. I have also discovered a new and direct method of treating functional equations of the first order, and of any number of variables, and this new method I have applied to the solution of differential and even of partial differential functional equations.

XXIV. *Some additional experiments and observations on the relation which subsists between the nervous and sanguiferous systems. By* A. P. Wilson Philip, *Physician in Worcester. Communicated by* T. Andrew Knight, *Esq. F. R. S.*

Read June 15, 1815.

In a paper which I had the honour to lay before the Royal Society, I observed that M. le Gallois founds his explanation of many of the phenomena which he describes in his Treatise, *Sur la Principe de la Vie*, &c. on the supposition that the circulation nearly ceases in any part when that portion of the spinal marrow from which it receives its nerves is destroyed. The accuracy of this supposition many circumstances led me to question. It is easy to subject it to the test of experiment.

Exp. 1. The spinal marrow of a frog was destroyed by moving, in various directions, a wire introduced into the spine by a hole made in the lowest part of it, and passed up into the brain. The animal was immediately deprived of sensibility and voluntary motion, and appeared to be quite dead. After it had lain in this state for several minutes, part of the web of one of the hind legs being brought into the field of a microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog. In making such experiments it is necessary to be aware, that handling and stretching the web tends to impair the vigour of the circulation in it. If this experiment is objected to on account of its being made on

an animal of cold blood, I may refer to the seventh and eighth experiments related in the paper above alluded to, in which the carotid and femoral arteries were found beating and performing the circulation after the spinal marrow had been wholly destroyed.

The labours of M. LE GALLOIS, by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have involved the subject in such seeming contradictions as, at first view, to have persuaded me that some of his experiments were inaccurate. On repeating many of them, however, I was convinced of their accuracy. In some the destruction of the cervical part of the spinal marrow immediately destroyed the function of the heart; yet in others the destruction, in a different way, of the same, or a larger portion of the spinal marrow, little affected it. In some, the greater part of the spinal marrow was destroyed without destroying the function of the heart; yet in others, after the spinal marrow had been divided, he found the function of the heart destroyed by the destruction of either half.

It was the confusion arising from these, and similar difficulties, that occasioned him to observe that he had almost as many results as experiments, and that he had resolved to abandon the investigation, when his explanation of the first of the foregoing difficulties, founded on the supposition which suggested the above experiment, presented itself to him. Had it occurred to him to compare this supposition with the latter difficulty, he would have doubted its accuracy.

The seeming contradictions which appear in the experiments of M. LE GALLOIS cannot be reconciled, except on prin-

ciples different from those hitherto assumed by physiologists. What these principles are, I have endeavoured to ascertain in the paper just mentioned. One part of the subject I left untouched, as it seemed at first sight to open too extensive a field of enquiry. It was evident in making the experiments related in that paper, that the laws which regulate the effects of stimuli applied to the brain and spinal marrow on the muscles of voluntary, and on those of involuntary motion, are very different. The following experiments point out more precisely in what this difference consists.

Exp. 2. Part of the cranium of a rabbit was removed, and a wire passed in various directions through the brain. I could not in this way in the least affect the muscles of voluntary motion, except when I made the wire approach those parts of the brain from which the spinal marrow and nerves originate. The muscles of voluntary motion were then thrown into violent spasms. I sliced off the whole of the upper and anterior part of the brain without affecting the muscles of voluntary motion. The knife only excited their action when it approached the source of the nerves.

Having deprived another rabbit of sensibility and voluntary motion by a blow on the occiput, that I might be enabled to judge of the effects which a stimulus applied to the brain would produce on the heart, I removed part of the cranium and laid open the thorax. The heart was found beating regularly. By passing a wire through the brain in any direction, the beats of the heart were accelerated and rendered stronger. I could not perceive that this effect was produced more powerfully when the wire was directed towards the source of the nerves, than when any other direction was given to it, provided it passed

through an equal portion of the brain. When an instrument was merely pressed gently on the surface of the brain, the effect was similar. When a pair of scissars, or any other thing of larger bulk than the wire was passed into the brain, the effect on the heart was greater than from the wire. It was still greater when the brain was wounded rapidly in many directions.

Exp. 3. Part of the cranium of a rabbit was removed, and after passing a knife through the brain in various directions towards the origin of the nerves, which excited the strongest spasms in the muscles of voluntary motion, the blood being absorbed by a sponge, I applied strong spirit of wine to the surface of the brain, and dropt it into the cuts, without at all affecting the muscles of voluntary motion. The upper part of the brain was then wholly removed, and the space filled with strong spirit of wine, but no spasms were excited in the muscles of voluntary motion.

Another rabbit was deprived of sensibility and voluntary motion by a blow on the occiput. Part of the cranium was then removed, the thorax laid open, and the heart found beating regularly. Spirit of wine was now applied to the surface of the brain, by which the frequency and force of the heart's beats were immediately increased. Several cuts were then made in the brain, and the spirit of wine dropt into them, by which the action of the heart was increased in a much greater degree. Spirit of wine increased the action of the heart more than any mechanical injury, which never produced the strong action in this organ, that it does in the muscles of voluntary motion.

This experiment was repeated with a watery infusion of

opium instead of spirit of wine; the result was in all respects the same, except that the action of the heart was less increased than by the spirit of wine.

Under the term brain, I mean to include the cerebellum as well as cerebrum. From many trials on rabbits made to ascertain the point, I could not perceive that the heart is more or less affected either by chemical or mechanical stimuli applied to the cerebellum than to the cerebrum; nor are the muscles of voluntary motion affected by wounding the cerebellum, except we approach the source of the spinal marrow and nerves. In some of my experiments, I thought that stimuli applied to the cerebellum affected the action of the heart rather more powerfully than when applied to the cerebrum; but this was contradicted by other experiments.

Exp. 4. I repeatedly cut off the head of a rabbit close to the occiput. For some time the trunk and limbs were affected with violent spasms. The cut end of the spinal marrow was so sensible that the slightest touch of a wire, after the spasms had subsided, immediately excited the action of the muscles of voluntary motion. The strongest spirit of wine and watery infusion of opium were applied to it, without producing the least effect on those muscles. The application, however, of stronger chemical stimuli, the nitric and muriatic acids, throw them into powerful contractions.

Having deprived a rabbit of sensation and voluntary motion, in an experiment already laid before the Society, I found that both spirit of wine and a watery infusion of opium applied to the spinal marrow, increase the action of the heart.

Exp. 5. I found both in rabbits and frogs that, after all stimuli applied either to the brain or spinal marrow had ceased to

produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimuli still increased the action of the heart; the former more than the latter.

Exp. 6. I tried, in every possible way, both by mechanical and chemical stimuli, and both before and after the sensibility was destroyed, to excite, through the brain or spinal marrow of rabbits and frogs, any irregular action in the heart which is so readily excited in the muscles of voluntary motion, but could not. Nor could I by sedatives, applied to the nervous system, occasion any irregular action in it. Its action was rendered quicker or slower, more or less frequent, stronger or weaker, but never irregular. The only instance in which irregular action was excited in the heart, was when its power was nearly destroyed by crushing the brain or spinal marrow.

Exp. 7. I found from many trials both on rabbits and frogs, that the excitement of the muscles of voluntary motion took place chiefly at the time the stimulus was applied to the brain or spinal marrow. It was generally necessary to move the instrument; thus applying it to a new surface in order to support the effect. The repeated contractions of the muscles of voluntary motion will sometimes continue, assuming the form of a fit, as long as the instrument remains in the brain, although it be kept as still as the motions of the animal will admit of. The increased action of the heart on the contrary, continued as long as the stimulus, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative, after the stimulant effect. The sedative effect was so far from being the consequence of the previous excitement, as many physiologists have supposed, that spirit of wine and mechanical stimuli, which produced no sedative

effect, but continued to stimulate the heart as long as they were applied, produced a much greater degree of excitement than tobacco, whose slight stimulant effect was quickly succeeded by a powerfully sedative one.

It appears from these experiments, that chemical stimuli, applied to the nervous system, exert a greater power over the heart than mechanical stimuli, while the latter exert a greater power over the muscles of voluntary motion than chemical stimuli; that both chemical and mechanical stimuli, applied to the nervous system, excite the heart, after they cease to produce any effect on the muscles of voluntary motion; that stimulating every part of the brain and spinal marrow equally affects the action of the heart, while the muscles of voluntary motion are only excited by stimuli applied to those parts of the nervous system from which the spinal marrow and nerves originate; that stimuli applied to the nervous system never excite irregular action of the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion; that their effect on these muscles is felt chiefly on their first application, but continues on the heart as long as the stimulus is applied. These differences in the effects of stimuli applied to the nervous system, on the muscles of voluntary and those of involuntary motion, which seem involved in so much obscurity, must be explained before we can be said to understand the relation which subsists between that system and the heart.

In the following part of this paper, I shall, in the first place, endeavour to trace the causes from which these differences arise; and afterwards to ascertain whether the power of the blood-vessels, like that of the heart, is indepen-

dent of the nervous system, and whether they are directly influenced by that system, or only through the medium of the heart.

It appeared to me probable, from many experiments, that the cause of chemical stimuli, applied to the nervous system, producing a greater effect on the heart than mechanical stimuli do, is, that the former from their nature act on a larger portion of the brain and spinal marrow. If this opinion is correct, the mechanical stimulus will be rendered the most powerful by confining the chemical to a smaller space than the mechanical stimulus occupies.

Exp. 8. Both in frogs and rabbits I applied to various parts of the brain and spinal marrow, and particularly to those parts from which the nerves originate, minute portions of strong spirit of wine, without at all influencing the action of the heart. When these small portions were applied to a great many parts, the heart began to beat more frequently. This of course was much the same thing as at once applying the spirit of wine to a larger part. We have seen in the foregoing experiments, that mechanical stimuli applied to any considerable portion of the nervous system, increase the action of the heart. It appears from the following experiments that we cannot affect the heart by mechanical stimuli confined to any small part either of the brain or spinal marrow.

Exp. 9. In a rabbit deprived of sensibility by a blow on the occiput, I wounded different small parts of the brain with a wire, particularly all those parts near which the nerves of the heart appear chiefly to originate; but could not affect the motion of this organ, while at the same time passing the wire

through any considerable portion of the brain immediately accelerated it.

Exp. 10. I laid open the cervical part of the spine of a rabbit, rendered insensible by a blow on the occiput, and repeatedly passed the wire transversely through the spinal marrow, without being able at all to affect the motion of the heart; but on passing the wire longitudinally, so as to bring it in contact with a larger portion of the spinal marrow, I found the motion of the heart immediately accelerated. On the same principle, when the wire was made to wound many minute portions of the brain and spinal marrow in quick succession, the action of the heart was increased. In another rabbit, I divided the spinal marrow at the occiput without at all affecting the heart.

Mr. CLIFT, in an account of experiments on the Carp, published in the Philosophical Transactions for this year, observes, that on dividing the spinal marrow at the occiput, the action of the heart was greatly accelerated for a few beats; but he divided the spinal marrow while the animal retained the power of the muscles of voluntary motion, which never fail to be called into action by wounding it, and whose action, by increasing the flow of blood, always accelerates the motion of the heart.*

Thus we see that neither chemical nor mechanical stimuli applied to the nervous system, affect the action of the heart,

* It is particularly satisfactory to me that Mr. CLIFT, on repeating my experiment, in which the spinal marrow was destroyed by a hot wire, found the same result in the carp, which I had done in rabbits and frogs. He did not ascertain whether the circulation continued after the destruction of the spinal marrow, but from this occasioning little or no diminution in the action of the heart, we can have little doubt of the continuance of the circulation.

unless they make their impression on a large part of this system. In the various experiments I have related, every part of the nervous system was stimulated individually, without the action of the heart being influenced, and the stimulus being the same, the force with which it acted on this organ, was always proportioned to the extent of surface to which it was applied. I could not find that it was of any importance what part of the brain was stimulated. Even stimulating the surface alone, either mechanically or chemically, immediately increased the action of the heart. The muscles of voluntary motion, on the contrary, we have seen, are wholly insensible to stimuli applied to the nervous system, except near the origin of the nerves. It is remarkable that while a rabbit perfectly retains its sensibility, and expresses great pain on any of the muscles being wounded, it exhibits no expression of pain whatever from the brain being sliced, until the knife approaches the origin of the nerves or spinal marrow.

Another circumstance, which appears to be of great importance in tracing the cause of the different effects of stimuli applied to the nervous system on the muscles of voluntary and involuntary motion, is, that the heart obeys a much less powerful stimulus than the muscles of voluntary motion do. We have seen that only the most powerful chemical stimulus affects them, while all that were tried readily influenced the action of the heart. Mechanical stimuli which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical stimuli, indeed, from their effects on the heart, we should consider the most powerful. But their greater effect on this organ is readily explained, by the influence of stimuli applied

to the nervous system on the heart, being proportioned to the extent of surface to which they are applied. It is evident that the stimulus can be applied to a greater extent of surface in the fluid than in the solid form. When the effect of the mechanical agent is rendered extreme and general on the nervous system, we find its influence on the heart far greater than that of any chemical agent I tried. From experiments I lately laid before the Society, it appears, that suddenly crushing any considerable part of the nervous system instantly destroys the power of the heart.

The conclusions then at which we arrive, are,—that the heart is excited by all stimuli applied to any considerable part of the nervous system, while the muscles of voluntary motion are only excited by intense stimuli applied to certain small parts of this system.

These facts being ascertained, the other differences observed in the effects of stimuli applied to the nervous system, on the heart and muscles of voluntary motion, are easily explained.

Irregular action of a muscle arises from stimuli acting partially, or at intervals, on its nerves, or on the particular part of the brain or spinal marrow, from which its nerves arise. But partial action of a stimulus on the nervous system, we have just seen, is incapable of exciting the heart, and while the stimulus is applied to any part of the nervous system, as all parts of this system seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion when it is moved from place to place in the brain. When the instrument is kept still after it is introduced into the brain, the action of the muscles of voluntary motion often ceases; its merely being in contact with the parts

of the nervous system which excite these muscles, not being sufficient to call them into action. It must bruise or lacerate to produce this effect. As the muscles of voluntary motion feel the impressions made on a very small part of the nervous system only, in proportion as this part is small, the impression must be great to affect them; but the heart, which is influenced through all parts of the nervous system, though not very powerfully through any one, feels all the impressions made on this system, provided they are made on a sufficiently extensive portion of it; thus, as long as the instrument remains in the brain, its stimulant effect on the heart continues.

It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. But it is evident, that the heart not being subject to stimuli whose action is confined to a small portion of this system, and being equally affected through all parts of it, must render it much less subject to irregular action; which may be one of the final causes of this organ, whose regular action is of such importance in the animal economy, being made subject to the whole, and not to any one part of the nervous system; and readily accounts for our not being able to produce irregular action in it, in the above experiments.

What has been said also explains why those, who have endeavoured to influence the heart by stimulating its nerves or the parts of the brain from which they seem chiefly to originate, have failed. When indeed the connection of the nerves of the heart is considered, it will be found to derive its nervous influence from every part of the nervous system, and not very

remarkably from any one part, a circumstance which particularly corresponds with the result of the foregoing experiments.

From the same facts we explain, why the heart is stimulated through the nervous system after the power of this system is so far weakened as no longer to convey the effect of the stimulus to the muscles of voluntary motion. As these obey stimuli applied to only one part of the nervous system, if the change in this part is not strong enough to produce the effect, it cannot be assisted by any other. Thus I have found by experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, because no one part suffers greatly, but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart.

It appears from the foregoing experiments, that the heart is influenced by every part of the nervous system; and in a former paper I pointed out why we have reason to believe that the intestines obey the same laws with the heart, although this cannot be so directly proved. From the situation of the ganglia compared with the whole of the experiments here alluded to, I think we cannot help believing, that their office is to combine the influence of the various parts of the nervous system, from which they receive nerves, and to send off nerves endowed with the combined influence of those parts.

Without some such means, it would be difficult to conceive how any organ should be influenced by every part of the nervous system. We cannot suppose that it receives nerves from every part of this system. Indeed we know, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the nervous system, others only by certain small parts of it. In the latter instances, we see nerves going from these small parts directly to the parts influenced. In the former instances, namely, where it is found that the part is influenced by all parts of the nervous system, we see no nerve going directly from any part of this system to the parts influenced; but we see these parts receiving nerves from ganglia, to which nerves from every part of this system are sent. It is therefore evident from direct experiment, that the nerves issuing from ganglia convey the influence of all the nerves which terminate in them, to the parts to which they send nerves; and consequently that this is one use of the ganglia; nor does there seem any reason to induce us to believe, that they have any other use. Thus it would appear, that the ganglia and nervous filaments connecting them, which have been called the great sympathetic nerve, are, if I may be allowed the expression, a channel of nervous influence flowing from every part of the brain and spinal marrow, from which those organs are supplied, which are subjected to the influence of the whole nervous system; those subjected to any particular part of this system, being supplied directly from that part. This view of the subject is consistent with the observations of anatomists, who remark that the great sympathetic has by no means the character of a nerve. Nothing surely can be

more different than this string of ganglia and a nerve, such as it passes directly from the brain and spinal marrow to the muscles of the trunk or limbs. It may also be worth remarking, that the nerves sent off from ganglia, have a very different appearance from those coming directly from the brain and spinal marrow. *Recherches Physiques sur la vie et la mort, &c. par M. Bichat.*

The question has been much agitated, why the will has no influence over the muscles of involuntary motion. It has been supposed that the ganglia intercept its influence, but we see in the above experiments, that the ganglia do not intercept the influence of either stimuli or sedatives applied to the nervous system. We can be at no loss to account for our want of power over these muscles, when we consider, that in their ordinary action, they obey stimuli over which we have no influence; and that, at all times, we neither see nor are otherwise conscious of their motions, and consequently cannot direct them.

I have endeavoured by the following experiments to ascertain, whether the power of the blood vessels is as independent of the nervous system, as that of the heart; and whether this system possesses over them the same kind of influence, as over the heart.

These experiments were made on the capillaries of the frog, which, from the extent and transparency of the web of its hind feet, and from its great tenacity of life, appeared the best subject for such experiments. It has been questioned, how far inferences drawn from experiments made on cold blooded animals, can be supposed to apply to those of

warm blood. Both Fontana and Dr. Monro observe, that in their experiments they found the system of both obeying the same laws. The experiments I have had occasion to lay before the Society, tend to confirm this observation; and I may say the same of all the experiments I have made on both sets of animals. There are certain circumstances in which they evidently differ, in all others they seem to agree. The following experiments ought not to be unnecessarily repeated, and as there is no part of the warm blooded animal on which they could be satisfactorily made except the mesentery, they would be attended with much greater suffering in this, than in the cold blooded animal. Some of them, from the warm blooded animal being less tenacious of life, could not be performed on it.

Exp. 11. A strong ligature was thrown round the neck of a frog, and the head cut off without any loss of blood; much loss of blood immediately destroys the circulation in the extremities. The spinal marrow was then destroyed by a wire. On bringing the web of one of the hind legs before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes. This experiment was repeated with the same result.

It appears from this experiment, that the blood vessels retain their power after the nervous system is wholly destroyed. In order to ascertain, how far the vessels can be stimulated through the nervous system, independently of any action of this system on the heart, it is necessary in the first place to determine, whether the vessels can support the motion of the blood independently of the heart.

Exp. 12. A ligature was thrown round all the vessels

attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind legs into the field of the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid.

In endeavouring to proceed farther, I found much difficulty. It was not only necessary, in order to ascertain the effect of stimuli applied to the nervous system on the vessels of the web, to remove the heart, and to lay open the cranium, but also to prevent the voluntary motions of the animal, which continually occurred, and never failed to accelerate the motion of the blood in the web.

Exp. 13. A frog was deprived of sensibility and voluntary motion, by the upper parts of the body being immersed in laudanum; part of the cranium was then removed, after a ligature had been thrown round the neck to prevent loss of blood. The thorax was now opened, and all the vessels attached to the heart included in a ligature. But notwithstanding this experiment was repeatedly performed with the greatest care, the circulation by all these preparatory means was so enfeebled, that although the blood still moved in the web, it was in so irregular and uncertain a way, that I never could arrive at any positive conclusion respecting the effect of the stimulus applied to the brain. After many fruitless attempts, therefore, I abandoned this mode of making the experiment.

Although the action both of the heart and the muscles of voluntary motion so influence the effect of stimuli applied to the brain, on the circulation in the foot, that, without wholly preventing the effect of both, no conclusion can be drawn, it is evident that the action of the latter cannot increase the effect of

sedatives; and the sedative lessening the power of the heart will not affect the result of the experiment, if it be made on the web of the frog. We have just seen, that the total ceasing of the action of the heart, does not for a considerable time affect the circulation in it. The following experiment appears to be decisive of the effect of the sedative, and of the stimulus, as far as this can be decisive, the action of the heart remaining. It is evident that the action of either stimulus or sedative is equally conclusive respecting the direct influence of the nervous system on the blood vessels.

Exp. 14. Part of the cranium of a frog was removed, the web of one of the hind legs brought into the field of the microscope, and the circulation in it observed. The animal was now rendered insensible by the immersion of the other hind leg in laudanum. The insensibility did not in the least affect the circulation in the web before the microscope. Spirit of wine was then applied to the brain with an evident increase of the velocity of the blood in the web. The same effect was produced in a less degree by watery solutions of opium and tobacco. After the tobacco had been applied for about half a minute, the motion of the blood was much less frequent than before its application. On washing off the tobacco the velocity of the blood increased, and was again lessened on applying it. This was repeated several times with the same effects. The following way of performing the experiment, is equally conclusive.

Exp. 15. A frog was rendered nearly insensible by having its back immersed in laudanum. A ligature was then thrown round the neck to prevent loss of blood, part of the cranium

removed, the web of one of the hind legs brought before the microscope, and the circulation in it, which was rapid, observed. A strong infusion of tobacco was then applied to the brain, with the effect of at first rendering the circulation more rapid. In about half a minute it became more languid, and soon stopped altogether. On the infusion of tobacco being washed off, the circulation returned and regained considerable vigour. The tobacco was several times applied to the brain and washed off, with the same effects. I may observe, that when the circulation in the web had almost ceased after the tobacco had been washed off, its velocity was immediately increased on applying spirit of wine to the brain.

Exp. 16. Analogous to what I had occasion to observe respecting the heart, I could never, either by chemical or mechanical agents, excite any irregular action in the blood vessels. Their action was only rendered more or less powerful.

The irregular appearances in the circulation in the web of a frog's foot, mentioned by Dr. THOMPSON, Professor of Military Surgery in the University of Edinburgh, in his Lectures on Inflammation lately published, and which he ascribes to inflammation, may be observed in any case, if the vessels be at all compressed in applying the foot to the microscope; and although they are not compressed, these appearances very generally occur when the circulation begins to fail. The blood will then stop and go on at intervals, and move backwards and forwards in the same vessel. I have often watched the capillaries from the commencement of inflammation to its greatest height, when the part is about wholly to lose its vital power, in the mesentery of a rabbit, the web of a frog's

foot, and the fins of fishes, without perceiving the least tendency to this irregular motion when the part viewed was so applied to the microscope as not to compress any of its vessels.*

The power of the blood vessels, like that of the heart, is capable of being directly destroyed through the medium of the nervous system.

Exp. 17. The web of one of the hind legs of a frog was brought into the field of the microscope, and while Mr. Hastings, who was good enough to assist me in this and the 14th experiment, observed the circulation, which was vigorous, I crushed the brain by the blow of a hammer. The vessels of the web instantly lost their power, the circulation ceasing. In a short time the blood again began to move, but with less force. This experiment was repeated with the same result. If the brain is not completely crushed, the blow increases the rapidity of the circulation in the web.

Exp. 18. The spine of a frog was laid open at the lower end, and a wire of nearly the same dimensions with its cavity, forced through it, as in M. le Gallois's experiments. The web of one of the hind legs was then brought into the field of the microscope, and the circulation in it was found to have wholly ceased. In another frog, as we have seen,† the spinal marrow was destroyed by the introduction, in the same way, of a wire much smaller than the cavity of the spine, and by its being moved in various directions. The frog soon appeared

* An account of these experiments is published in the introduction to the second part of my Treatise on Febrile Diseases, and a plate given representing the state of the vessels in the different stages of inflammation.

† See Experiment 1.

to be quite dead, but the circulation in the web was found to be vigorous.

From the foregoing experiments and observations, it appears,

1. That the laws which regulate the effects of stimuli, applied to the nervous system, on the muscles of voluntary and involuntary motion, are different. Exp. 2, 3, 4, 5, 6, 7.

2. That both mechanical and chemical stimuli, applied to any considerable portion of the nervous system, increase the action of the heart. Exp. 2, 3.

3. That neither mechanical nor chemical stimuli applied to the nervous system, excite the muscles of voluntary motion, unless they are applied near to the origin of the nerves and spinal marrow. Exp. 2, 3, 4.

4. That mechanical stimuli applied to the nervous system, are better fitted to excite the muscles of voluntary motion, and chemical stimuli, those of involuntary motion. Exp. 2, 3, 4.

5. That after all stimuli, applied to the nervous system, fail to excite the muscles of voluntary motion, both mechanical and chemical stimuli, so applied, still excite the heart. Exp. 5.

6. That both mechanical and chemical stimuli applied to the nervous system, excite irregular action in the muscles of voluntary motion. Exp. 2, 3, &c.

7. That neither excite irregular action in the heart, nor is its action rendered irregular by sedatives, unless a blow which crushes the brain be regarded as a sedative. Exp. 6.

8. That the excitement of the muscles of voluntary motion takes place chiefly at the moment at which the stimulus is

applied to the nervous system, that of the heart continues as long as the stimulus is applied. Exp. 7.

9. That the muscles of voluntary motion are excited by stimuli applied to very minute parts of the nervous system. Exp. 2, 3, 4.

10. That no stimulus applied to any minute part of the nervous system, can excite the heart. Exp. 8, 9, 10.

11. That the heart obeys a much less powerful stimulus than the muscles of voluntary motion. Exp. 3, 4, &c. and observations after Exp. 10.

12. That the facts expressed in the three last sentences 9, 10, 11, afford an easy explanation of those expressed in the preceding sentences. See the observations after Exp. 10.

13. That the power of the blood vessels, like that of the heart, is independent of the nervous system. Exp. 1, 11.

14. That the blood vessels can support the motion of the blood after the heart is removed. Exp. 12.

15. That the blood vessels are directly influenced through the nervous system in the same way that the heart is. Exp. 14, 15.

16. That analogous to what we observe in the heart, no stimulus or sedative applied to the nervous system, excites irregular action in the blood vessels. Exp. 16.

17. That the power of the blood vessels, like that of the heart, may be destroyed through the nervous system. Exp. 17, 18.

18. That the office of the ganglia is to combine the influence of the various parts of the nervous system, from which

they receive nerves, and to send off nerves endowed with the combined influence of those parts.

19. That the will has no influence over the muscles of involuntary motion, because in their ordinary action they obey stimuli, over which we have no influence, and because at all times we neither see, nor are otherwise conscious of, their motions; and consequently cannot direct them.

20. That we have reason to believe that the division of the encephalon into the cerebrum and cerebellum, relates to the sensorial functions, since it does not appear to relate to the nervous functions, the muscles of voluntary and those of involuntary motion being influenced in the same way by both.

21. That the sedative effect is not the consequence of previous excitement, but the effect of a certain class of agents. Exp. 7.

ADDITIONS AND CORRECTIONS.

Page 99, line 5, *for* Protagoras, *read* Protogenes
186, last line, *for* southerly, *read* northerly
204, line 4, *for* heat, *read* iodine
272, line 17, for *about* 10°, read *between* 12° *and* 14°
275, line 18, *for* 39°, *read* 37° 30′
277, line 17, *for* nearly $\frac{1}{200}$, *read* less than $\frac{1}{820}$
Id. lines 21 and 23, *for* 141°, *read* 141° 53′
278, line 12, at the word crystallized, insert the following as a note: *The interrupting stratum is crystallized in a different manner from the rest of the rhomboid. The position of its axes with respect to those of the rhomboid, and the singular optical phenomena which arise from this cause, will be described in another paper.*
288, Note, line 4, *for* parallel, *read* perpendicular

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